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# *THE BASAL KATABOLISM OF CATTLE AND OTHER SPECIES*

By Henry Prentiss Armsby, J. August Fries and Winfred Waite Braman

Institute of Animal Nutrition, The Pennsylvania State College

Communicated by R. Pearl, December 13, 1917

The basal katabolism of herbivora and especially of ruminants, unlike that of man or carnivora, cannot well be measured in the fasting state on account of the relatively large amount of feed always present in the alimentary canal of the former species. It may, however, be determined indirectly in the manner described by the authors[1,2,3] by measuring the total metabolism upon two different amounts of the same ration and from these data computing the level to which the metabolism would be reduced were all feed withdrawn. For example, a steer receiving two different amounts of the same mixed ration gave the following results:

| | DRY MATTER EATEN DAILY | DAILY HEAT PRODUCTION |
|---|---|---|
| | *kgms.* | *calories* |
| Period 2 | 9.146 | 16,511 |
| Period 1 | 4.463 | 10,905 |
| Difference | 4.683 | 5,606 |
| Heat increment per kilogram of dry matter | | 1,197 |

Evidently, out of the total metabolism of 10905 Calories in Period 1, 1197 × 4.463 = 5342 Calories may be regarded as the heat production caused by the 4.463 kgm. of dry matter eaten while the remainder, 5563 Calories is the basal katabolism.

Our investigations upon the metabolism of cattle, which have been published elsewhere[3] afford data for computing in the manner just illustrated the basal katabolism of ten unfattened steers in twenty-seven experiments. In view of the very striking effect of standing in increasing the metabolism of cattle the basal katabolism per 24 hours has been computed separately from the observed rate of heat production during the intervals of lying and standing, respectively, and also for 12 hours standing and 12 hours lying per day, assumed as representing average conditions.

As was to be expected, the basal katabolism increased with the size of the animal but with very considerable fluctuations. The graphs of the results indicate an equally close relation of the basal katabolism with the weight and with the two-thirds power of the weight (computed body surface) and this conclusion is confirmed by a comparison of the coefficients of correlation as follows:

*Coefficients of correlation*

| | WITH LIVE WEIGHT | WITH 2/3 POWER OF LIVE WEIGHT |
|---|---|---|
| Basal katabolism, lying 24 hours | 0.8655 ± 0.0326 | 0.9032 ± 0.0239 |
| Basal katabolism, standing 12 hours | 0.8733 ± 0.0308 | 0.8710 ± 0.0313 |
| Basal katabolism, standing 24 hours | 0.8548 ± 0.0350 | 0.8250 ± 0.0415 |

Computing the basal katabolism per square meter of body surface as estimated by Moulton's formulae[4] viz.,

$$\text{For unfattened animals } S = 0.1186\ W^{5/8}$$
$$\text{For fattened animals } S = 0.158\ W^{5/9}$$

the following results were obtained.

*Basal katabolism of cattle per square meter of body surface*

| | LYING 24 HOURS | STANDING 12 HOURS | STANDING 24 HOURS |
|---|---|---|---|
| Mean, Calories | 964.0 | 1173.0 | 1365.0 |
| Probable error of mean, Calories | ± 24.0 | ± 21.4 | ± 25.7 |
| Probable error of single result, Calories | ±124.8 | ±110.9 | ±133.6 |
| Standard deviation, Calories | 185.1 ± 17.0 | 164.5 ± 15.1 | 198.0 ± 18.2 |
| Coefficient of variability | 0.1920 | 0.1462 | 0.1451 |

A positive correlation of the basal katabolism per square meter body surface with the live weight was also found as follows:

*Coefficients of correlation with live weight*

| Basal katabolism per square meter | |
|---|---|
| Lying 24 hours | 0.5375 ± 0.0923 |
| Standing 12 hours | 0.3666 ± 0.1124 |
| Standing 24 hours | 0.2405 ± 0.1223 |

The results show the marked influence of standing upon the metabolism of cattle, the mean 24 hour basal katabolism lying, standing 12 hours and standing 24 hours being in the proportion of 100 : 121 : 141, the differences largely exceeding the probable errors. Computing, from the results per square meter of surface, the basal katabolism for 12 hours standing and 12 hours lying gives as the maintenance requirement for a 1000-pound steer 5918 ± 560 Calories.

The results for the basal katabolism of man reported by Benedict, Emmes, Roth and Smith,[5] and by Means[6] present much the same picture as ours upon cattle with the exception of a much lower variability.

*Coefficients of correlation*

| | WITH BODY WEIGHT | WITH BODY SURFACE |
|---|---|---|
| Total basal katabolism | | |
| 98 men | 0.7263 ± 0.0320 | 0.7747 ± 0.0272 |
| 75 women | 0.7759 ± 0.0310 | 0.7447 ± 0.0347 |

*Daily basal katabolism of men and women per square meter of surface*

| | MEN | WOMEN |
|---|---|---|
| Mean Calories | 830.0 | 768.0 |
| Probable error of mean, Calories | ± 4.3 | ± 4.9 |
| Probable error of single result, Calories | ±42.3 | ±42.8 |
| Standard deviation, Calories | 62.7 ±3.0 | 63.5 ±3.1 |
| Coefficient of variability | 0.0755 | 0.0827 |

Correcting for the error shown by D. and E. F. DuBois[7] to be incident to the use of the Meeh formula, the means for men and women are as follows:

*Corrected daily basal katabolism of men and women per square meter of body surface*

| | MEN | WOMEN |
|---|---|---|
| Means, Calories | 935.0 | 886.0 |
| Probable error of mean | ± 4.8 | ± 5.8 |
| Probable error of single result | ±47.5 | ±49.4 |

Including the data obtained by Meissl, Strohmer, and Lorenz[8] by Tangl[9] and by Fingerling, Köhler and Reinhardt[10] for swine and by Zuntz and Hagermann[11] for the horse, the following comparison of species may be made.

*Mean daily basal katabolism per square meter of body surface*

| | |
|---|---|
| Men (complete muscular rest) | 935 ± 5 |
| Women (complete muscular rest) | 886 ± 6 |
| Cattle (lying) | 964 ± 24 |
| Hogs (lying) | 1078 ± ? |
| Horse (standing quietly) | 948 ± ? |

Considering the nature of the results they show a rather striking degree of uniformity and tend to confirm the conclusions of E. Voit[12] that the basal katabolism of different species of animals is substantially proportional to their body surface. It may be surmised that the exceptional result with the hog is due to the imperfect data available for computing the body surface of this species.

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[1] Armsby, *Washington, D. C., U. S. Dept. Agric., Bur. Anim. Indust., Bul.* **142,** 1912.

[2] Armsby and Fries, *Ibid., Bul.* **128,** 1911.

[3] Armsby and Fries, *J. Agric. Res., Washington,* **3,** 1915, (435); **10,** 1917, (599); **11,** 1917.

[4] Moulton, *J. Biol. Chem., New York,* **24,** 1916, (299).

[5] Benedict, Emmes, Roth and Smith, *Ibid.,* **18,** 1914, (139).

[6] Means, *Ibid.,* **21,** 1915, (263).

[7] DuBois and DuBois, *Arch. Inter. Med.,* **15,** 1915, (868).

[8] Meissl, Strohmer and Lorenz, *Zs. Biol., München* **22,** 1886, (63).

[9] Tangl, *Biochem. Zs.,* **44,** 1912, (252).

[10] Fingerling, Köhler and Reinhardt, *Landw. Versuchstat, Berlin,* **84,** 1914, (149).

[11] Zuntz and Hageman, *Landw. Jahrb., Berlin,* **27,** 1898, Ergzbd. III, (284).

[12] Voit, E., *Zs. Biol., München,* **41,** 1901, (113).

---

# *THE LOCATION OF THE SUN'S MAGNETIC AXIS*

By F. H. Seares, A. van Maanen, and F. Ellerman

Mount Wilson Solar Observatory, Carnegie Institution of Washington

Communicated by G. E. Hale, November 26, 1917

The discovery by Mr. Hale in 1913 of a general magnetic field[1] surrounding the sun raised at once questions of great interest. First among these was the character of the field and the variation of its intensity over the solar surface. A preliminary investigation showed that approximately the sun may be regarded as a uniformly magnetized sphere, its axis coinciding with the axis of rotation. The minuteness of the observed quantities and the difficulties experienced in their measurement necessitated the provisional acceptance of this simple hypothesis; nevertheless, well-known peculiarities of the earth's magnetic field suggested that the solar field might deviate from that of a spherical magnet, and that its axis might be inclined to the rotation axis by an amount susceptible of measurement by special series of observations. This communication is concerned with the latter of these questions, namely, the position of the magnetic axis.

Observations of the sun's field are made by placing the slit of the spectrograph in coincidence with the central solar meridian. A compound quarter-wave plate and a Nicol prism just outside the slit serve as an analyzer, the observed effect being a minute displacement of an appropriately chosen spectral line. The amount of the displacement varies with the inclination of the lines of force to the line of sight, in other words, with the position of the sun's magnetic axis, the heliographic latitude of the point observed, and the distance of the observer from the plane of the sun's equator. If the field be that of a uniformly magnetized sphere,[2]

$$k\Delta = \{3 \sin (2\phi - D) + \sin D\} \cos i + \{3 \cos (2\phi - D) + \cos D\} \sin i \cos \lambda \tag{1}$$

in which

$\Delta$ = displacement of spectral line;
$\phi$ = heliographic latitude of point observed;
$D$ = angular deviation of observer from plane of sun's equator;
$i$ = inclination of sun's magnetic axis to axis of rotation;
$\lambda$ = heliographic longitude of north magnetic pole referred to central meridian;
$k$ = constant depending on the units and the behavior of the line in a field of known intensity.

For $i = 0$, equation (1) reduces to

$$k\Delta = 3 \sin (2\phi - D) + \sin D, \tag{2}$$

and, if $D$ also is zero, to

$$k\Delta = 3 \sin 2\phi. \tag{3}$$

Since the maximum value of $D$ is about 7°, equation (2) differs but little from (3). The displacement curves derived by Mr. Hale from preliminary observations agreed substantially with these equations, whence it follows that $i$ must also be small and that the difference between equations (1) and (2), which represents the influence of $i$, is a quantity of the second order.

When $\Delta$ is expressed in thousandths of a millimeter, $k$, for the lines observed, is of the order of unity. The maximum displacement, by equation (3), is therefore 3 or 4 $\mu$ (about 0.001 A). To determine the position of the magnetic axis, quantities of the order of 0.5 $\mu$ must accordingly be evaluated. This indicates sufficiently the nature of the problem and the degree of precision that had to be attained. It was evident from the beginning that a long and carefully executed series of observations would be required for a successful attack on the problem.

The original investigation by Mr. Hale was based on only four lines. Later observations have increased the number known to be affected by the sun's field to 30, for 18 of which results were communicated at the Atlanta meeting of the American Astronomical Society in December, 1913. For the investigation here described three chromium lines, $\lambda\lambda$ 5247, 5300, 5329 were selected, which are of special suitability for measurement because of intensity (2 and 3), location in the spectrum, and magnitude of displacement.

From June 8 to September 23, 1914, these lines were photographed daily under the direction of Ellerman, with almost no break in the series. The circumstances were most favorable owing to the small number of sun-spots, whose magnetic fields, many times the intensity of the underlying field of the sun, seriously complicate the investigation. Because of advantages connected with the numerical solution and the necessity of limiting what at best could be only a very laborious undertaking, the observations were confined to the zone 45°N–45°S. Twelve spectrograms with exposures of 10 to 30 minutes constituted the normal observing program for each day. For 63 of the days the photographs have been completely measured by van Maanen, who has assumed the responsibility for this part of the undertaking. More than 2000 sets of measures were required, each involving about a hundred settings of the micrometer.

In measures of minute displacements of spectral lines, systematic errors are always to be suspected, as well as the influence of prejudice arising from a knowledge of the results that will satisfy a given hypothesis. Such systematic errors as may have entered in the present case probably affect only the constant $k$, which varies from line to line but does not enter into the determination of the position of the magnetic axis.

To exclude the influence of prejudice, the procedure devised by Mr. Hale has been followed here. The limited zone of heliographic latitude covered by a single spectrogram may lie in the northern hemisphere, where the displacements are positive, or in the southern hemisphere, where they are negative; or it may extend over the equator and thus show only very small displacements, some negative and some positive. The measurer has rarely known in advance the latitudes covered by any spectrogram. Further, the above distribution of algebraic signs presupposes that the photograph has been made with the compound quarter-wave plate in its normal position. Since the inversion of the plate reverses the signs of the displacements, its position, as a final precaution, has been varied at random by the observer, and the measurer has not known the position used for a given photograph until after his settings were finished.

The data have been treated as follows: Each displacement affords an equation of condition of the form (1) for the determination of the unknowns $k$, $i$, and $\lambda$. The longitude of the magnetic pole, $\lambda$, involves an epoch, $t_0$, when the pole was on the central meridian, and the period, $P$, in which the magnetic axis revolves around the axis of rotation. For a single day we may assume $\lambda$ to be constant, which leads us to discuss separately the observations for each day and for each line, thus deriving values for two new unknowns, $x$ and $y$, which are functions of $k$, $i$, and $\lambda$. The analysis of $x$ and $y$ for the whole series of days then determines $k$, $i$, $P$, and $t_0$.

Equation (1) may be written

$$Ax + By = \Delta. \tag{4}$$

$A$ and $B$ are the bracketed expressions of (1), including only known quantities, and

$$x = k^{-1} \cos i, \qquad y = k^{-1} \sin i \cos \lambda \tag{5}$$

whence

$$Y = y/x = \tan i \cos \lambda. \tag{6}$$

About 50 values of $\Delta$ were available for each line on each day. Means were found for groups of 5 or 6 adjoining displacements, thus giving 8 or 10 observation equations of the form (4) for a least-squares determination of $x$ and $y$.

The individual values of $\Delta$ for September 2, 1914, a series of average weight, are plotted in Fig. 1 against the latitudes as abscissae. The close agreement with a sine curve of the type of equation (3) appears at a glance. The calculated displacement-curves corresponding to the values of $x$ and $y$ derived from these data, are also shown in the figure. Their ordinates for $\phi = 0$, namely, +0.8, +1.0, and +0.5 $\mu$, respectively, are of the order of the small quantities which differentiate the displacement-curve (1) from the curve (2) and indicate the precision with which the curves must be located in order to determine the value of $i$.

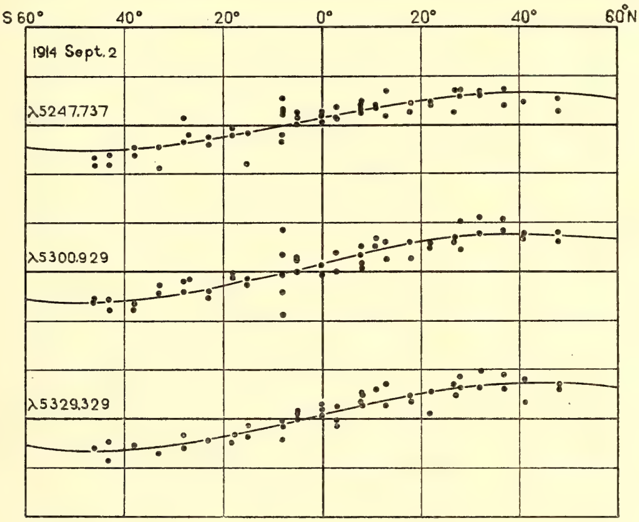

FIG. 1. DISPLACEMENT-CURVES FOR 1914, SEPTEMBER 2

Abscissae are heliographic latitudes. Ordinates are displacements, the scale being 1 division of diagram = 0.005 mm. The curves, which correspond to equation (1), have been derived from the observed values of $\Delta$. Their ordinates for $\varphi = 0$ represent the combined influence of $k$, $D$, $i$, and $\lambda$. These data for the three lines give $Y = \tan i \cos \lambda = +0.213$ which is plotted as a single point in Fig. 2, together with similar values of $Y$ for each of the other dates.

Having found $x$ and $y$ from each line for each day, the results were combined by (6) to form weighted mean values of $Y$, which were then plotted with the times as abscissae. These should define a sine curve whose amplitude and period are, respectively, tan $i$ and $P$. The individual points are reproduced in Fig. 2, from which approximations for $i$, $P$ and $t_0$ were easily derived. The final values and their probable errors

$$i = 6\overset{\circ}{.}2 \pm 0\overset{\circ}{.}4, \qquad P = 31.79 \pm 0.31 \text{ days}$$
$$t_0 = 1914, \text{ June } 25.31 \pm 0.42 \text{ days, G. M. T.}$$

were calculated by a least-squares solution giving differential corrections to the approximate values read from the curve.

The theoretical curve for $\tan i \cos \lambda$ corresponding to these elements is also shown in Fig. 2. So far as accidental errors are concerned, the representation is very satisfactory, for the amplitude of the curve corresponds to an extreme difference of only 1 $\mu$ in the position of the individual displacement curves. There is some evidence of a gradual increase in $\tan i$; but no emphasis is placed on the phenomenon. Whether it is real or whether it is due to errors of measurement and the influence of spots and the 'dark markings' of the Greenwich observers cannot now be determined. Days on which spots were near the central meridian were generally avoided. The magnetic fields surrounding some of the 'dark markings' noted at a later time (our attention was first directed to them in 1915) were measured and found to be two or three times as intense as the sun's general field; but their existence and influence upon the measures here discussed cannot now be traced.

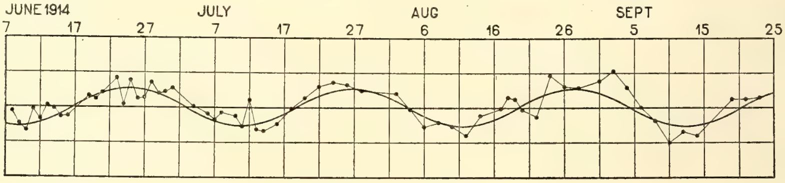

FIG. 2. THE CURVE $Y = \tan i \cos \lambda$

Each plotted point is derived from data for a single day similar to that illustrated in Fig. 1. Approximate values of $i$, $P$, and $t_0$ were read from a provisional curve. Differential corrections derived by a least-squares solution gave the final values, which correspond to the curve shown in the figure.

The result for $P$ is of much interest in relation to the rotation period of the sun. For the reversing layer this ranges from 26.4 days (synodic period) at the equator to about 30.5 days at 45°, the value for $P$ not being equalled until approximately 55° is reached. Hence for latitudes lower than this limit, the magnetic axis in its motion about the axis of rotation appears to lag behind the reversing layer. The bearing of this circumstance upon the physical constitution of the sun must await further elucidation. In the meantime, however, it should be noted that $P$ may not be constant. The present series of measures, at any rate, is insufficient to establish its freedom from fluctuations; and none of the earlier observations, which were made with other purposes in view, are adapted to the investigation.

The fact that the observed values of $Y$ agree satisfactorily with the periodic function $\tan i \cos \lambda$ does more than show that $i$ is not zero; it affords a most convincing demonstration of the existence of the magnetic field itself. The measures of each of the three lines, on each of the sixty odd days, define a displacement-curve which is an independent confirmation of the existence and general character of the field; but the fact that the curves for the separate days are so related to each other as to satisfy equation (6) increases enormously the weight of the conclusion and seems to exclude the possibility that it should be the consequence of an obscure and unsuspected systematic error.

We desire to express our great obligation to Miss Wolfe of the Computing Division who has executed most efficiently the numerous least-squares solutions required for the discussion of the data.

[1] Hale, G. E., *Terr. Mag., Baltimore*, **17**, 1912 (173–178); *Mt. Wilson Contr.* No. 71, *Astroph. J., Chicago*, **38**, 1913, (27–98).

[2] Seares, F. H., *Mt. Wilson Contr.* No. 72, *Astroph. J., Chicago*, **38**, 1913, (99–125).

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# *RESONANCE AND IONIZATION POTENTIALS FOR ELECTRONS IN CADMIUM, ZINC, AND POTASSIUM VAPORS*[1]

By John T. Tate and Paul D. Foote

University of Minnesota and Bureau of Standards

Communicated by R. A. Millikan, December 19, 1917

It has been shown by Franck and Hertz and others that there are, for electrons accelerated through gases or vapors, certain definite potentials at which there is a large transfer of energy from the electron to the atom, as evidenced by the emission at these potentials of radiations characteristic of the gas atom. This is to be expected on purely mechanical grounds since no considerable transfer of energy from the light electron to the relatively heavy gas atom can take place except when the time of encounter between electron and atom bears some simple relation to the characteristic period of one of the vibrational degrees of freedom in the atom. It is therefore to be expected that there will be a critical potential corresponding to each absorption line of the gas and that at this potential the electrons will give up their energy to the gas and cause the emission of a radiation of the frequency of the corresponding absorption line.

Two types of inelastic encounter between electrons and gas atoms have been observed. One of these results in the emission of a radiation of a single frequency, without ionization of the gas, while the other ionizes the gas and causes it to emit a composite spectrum of radiations. The potential giving the first type of encounter may be termed a resonance potential, that giving the second type an ionization potential.

The present paper is an account of an experimental determination of the resonance and ionization potentials in cadmium zinc, and potassium vapors.

The method employed was that described by Tate[2] for the determination of critical potentials in mercury vapor and the apparatus was similar to that used by us[3] in our work on sodium vapor.

The results obtained are shown in the accompanying table which also contains for the sake of completeness the results, already published, for sodium vapor.

| METAL | RESONANCE POTENTIAL (VOLTS) | | WAVE LENGTH OF RADIATION | IONIZATION POTENTIAL (VOLTS) | | LIMITING WAVE LENGTH OF SERIES |
|---|---|---|---|---|---|---|
| | Observed | Calculated | Å | Observed | Calculated | Å |
| Cadmium | 3.88 | 3.79 | 3260.17 | 8.92 | 8.97 | 1378.69 |
| Zinc | 4.10 | 4.02 | 3075.99 | 9.50 | 9.37 | 1319.95 |
| Potassium | 1.55 | 1.65 | 7685.0 | 4.10 | 4.33 | 2856.0 |
| Sodium | 2.12 | 2.10 | 5893.0 | 5.13 | 5.13 | 2413.0 |

The curves for zinc vapor indicate the possible presence of a secondary inelastic encounter, apparently without ionization, at 2.3 volts; but further work is in progress to make sure of this point.

It will be seen that in all cases the results agree within the limits of experimental error with the values as calculated from the quantum relation $h\nu = eV$, where $\nu$ is the frequency of the single radiation in the case of resonance potentials or the limiting frequency of the series of radiations in the case of ionization potentials.

It is to be expected that the frequencies corresponding to the ionization potentials should be the long wave-length limit of the photo-sensibility of the vapor in question. Experiments are now in progress to determine whether or not this is so.

Concerning the question of the cause for the appearance of ionization in metallic vapors at applied potentials much lower than the ionization potential it should be remarked that in no case was there an observable decrease in the value of the resonance potential even under conditions which permit of the appearance of the complete spectrum at low potentials. It is therefore thought that this setting in of ionization at applied potentials much lower than the ionization potential is due rather to the presence of electrons having high initial velocities than to a decrease in the value of the critical potentials. The curves obtained with potassium vapor clearly show the presence of a large number of electrons having initial velocities corresponding to 1.6 volts.

[1] Abstract of a paper presented before the American Physical Society, December 1, 1917.

[2] Tate, *Physic. Rev. Ithaca, N. Y.*, **10**, 1917, (81).

[3] Tate and Foote, *Washington, J. Acad. Sci.*, **7**, 1917, (517).

# THE VALIDITY OF THE EQUATION $P = T\, dv/dT$ IN THERMO-ELECTRICITY

By Edwin H. Hall

Jefferson Physical Laboratory, Harvard University

Read before the Academy, November 21, 1917

The equation $P = T\, dv/dT$, in which $P$ stands for the Peltier effect and $v$ for the Volta effect between any two metals, was first obtained, I believe, by Lord Kelvin, comparatively late in his life. He derived it from a course of argument involving the imagined performance of a certain isothermal cycle. It was derived later by O. W. Richardson through a series of steps including his theory of thermionic emission and the imagined performance of a reversible cycle taking electrons from a metal at one temperature and putting them back into the same metal at a different temperature.

When Kelvin came to test his formula by the experimental data available to him he found no support for it. In fact, it is said that "the experimental verification failed by a thousandfold."

During the last few years experiments made by various skillful investigators measuring $dv/dT$ in high vacua have perhaps reduced the ratio of the two sides of the professed equation to fifty; but it still remains so large that it would be considered a complete refutation of the equation's pretensions, if it were not possible to find an escape from this conclusion by dwelling upon the great difficulty of the experimental investigation.

I have not, until recently, given much attention to this equation; but two or three months ago my colleague, Professor Bridgman, who has lately been studying thermo-electricity intensively, put into my hands for criticism a manuscript in which, going over with modifications of his own the Kelvin method and the Richardson method, he had in both cases reached their conclusion. Studying Bridgman's manuscript I am, on the contrary, strongly inclined to the opinion that in each method the proof fails. As Professor Bridgman is now too much occupied with government work to give adequate attention to the question at issue, he has given me permission to present this question here.

I cannot reproduce at length either of the arguments. I can only point out what I believe to be the weak link in each. In the first, which was suggested by that of Kelvin, Bridgman assumes that when, by an applied e.m.f., electricity is carried along a metallic path from metal A, constituting one plate of a condenser, to metal B, constituting the other plate, the only heat phenomenon, except the negligible resistance effect, is the production or absorption of heat at the junction of the two metals, the ordinary Peltier effect. This assumption, which is vital to the argument, I question.

Let $n_e$ be the number of free electrons per unit volume of a metal and $n_i$

the number of positive ions, atoms lacking each an electron. Within the interior of a metal in electrical equilibrium we must have $n_e = n_i$. At the surface, if the metal has a static charge, we do not have such equality. Now, according to my view, the free electrons and the positive ions, which are the products of the ionization of the atoms, obey the mass-law, so that everywhere in a metal we have

$$n_e \times n_i = a, \textit{ constant.}$$

If this view is sound, when we take electrons from one plate of a condenser and convey them to the other plate, we thereby disturb the electrical equilibrium in each metal. If one metal loses, for example, $q$ electrons, we cannot have mass-law equilibrium in this metal until sufficient new ionization occurs therein to make the total number of free electrons if this metal only $\frac{1}{2}\ q$ less, and the total number of positive ions $\frac{1}{2}\ q$ more, than at first. In the other metal the converse operation must take place, re-association occurring there until the number of free electrons is $\frac{1}{2}\ q$ more and the number of positive ions $\frac{1}{2}\ q$ less than before. These processes of ionization and of re-association would balance each other in heat production if the two plates of the condenser were of the same metal, but otherwise they do not. Of course, wherever in the metals the ionization and the re-association primarily occur the result will presently appear at the surface, since any excess or deficiency of electrons in the interior of a metal must correct itself at the expense of the surface. Practically, then, we may regard the ionization and the re-association as occurring at the surface only.

Bridgman says that the failure of the equation $P = T\ dv/dT$ to bear the experimental test led Kelvin "to infer the existence of reversible heating effects at the surface of a metal when a charge is added to or subtracted from the surface, just as he had previously inferred the existence of the Thomson heat." Bridgman has thought that this inference was not justified and that sufficiently careful experiments would verify the equation. I think, on the contrary, that Kelvin's inference was sound, that the ionization and re-association phenomena which I have indicated are precisely the reversible heating effects at the surface which he infers, but does not visualize, and that the equation in question will never be justified by experiment.

The other line of argument begins with a metal in equilibrium with a surrounding electron atmosphere contained within an enclosure otherwise vacuous. The following quotations are from the second chapter of Richardson's *Emission of Electricity From Hot Bodies:*

"In this steady state there will be a definite number $n$ [of electrons] per unit volume, on the average, in the vacuous enclosure, and they will exert a definite pressure $p$. If the enclosure is provided with a cylindrical extension in which an insulating piston can move backwards and forwards, this pressure $p$ can be made do work against an external force." "The relation between the pressure of these electrons and the temperature of the enclosure can be found by an application of the second law of thermodynamics." "All we need is an expression for $dS$, the increment in the entropy caused by motion of the piston. If $\varphi$ is the change in the energy of the system which accompanies the transference of each electron from the hot body to the surrounding enclosure, then

$$dS = \frac{1}{T}\,[d(n\ v\ \varphi) + pdv]," \text{ etc.}$$

In writing this equation, which is fundamental to his argument, Richardson treats the case of a metal emitting electrons precisely as one treats the case of a body of water giving off steam to push against a piston. That is, he treats the emission of electrons as a process strictly comparable with evaporation. But there is an important difference between the two processes. In evaporation the thing given off is of the same substance as that left behind. In the emission of electrons this is not true. Evaporation leaves the constitution of the remaining liquid unchanged. Emission of electrons continually changes the constitution of the emitting metal, unless other electrons are put into the body to make good the loss. When a body emits a certain mass $m$ of electrons under the conditions described by Richardson, the system under discussion takes in something more than heat energy; it takes in *substance*, the mass $m$ of electrons. There is no analogue to this in the process of evaporation, and it remains to be shown that the equation which I have quoted from Richardson, an equation that holds beyond question for the case of evaporation, holds also for the case of emission of electrons.

One cannot, according to my view, meet this difficulty by supposing the body of metal made very large, so large that the static charge produced on it by the emission of a mass $m$ of electrons without compensation would be negligibly small. For, if the loss of the electrons is not made good, the mass-law, requiring that $n_e \times n_i$ shall remain constant within the metal, will cause ionization there proportional to $m$, without regard to the amount of the metal; and this ionization will introduce a consumption of heat for which Richardson has made, I think, no provision.

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## *ON THE EQUATIONS OF THE RECTANGULAR INTERFEROMETER*

By Carl Barus

Department of Physics, Brown University

Communicated December 8, 1917

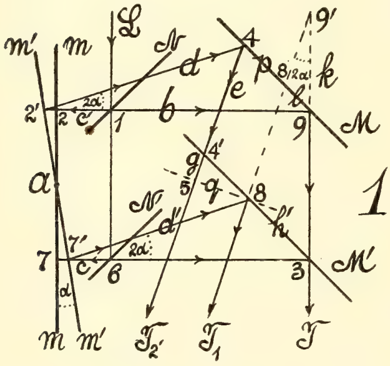
1. *Auxiliary Mirror.*—It is desirable to deduce the fundamental equations more rigorously than has heretofore[1] seemed necessary. Figure 1 is supplied for this purpose, and represents the more sensitive case, where in addition to the mirrors $M$,$M'$, $N$,$N'$ (all but $M$ being necessarily half silvers), there is an

auxiliary mirror *mm*, capable of rotation (angle $\alpha$) about a vertical axis $A$. The mirrors $M$–$N'$ in their original position are conveniently at 45° to the rays of light,, while *mm* is normal to them. Light arriving at $L$ is thus separated by the half silver $N$ at 1, into the two components 1,2,1,9,3,$T$ and 1,6,7,6,3,$T$, interfering in the telescope at $T$.

When *mm* is rotated over a small angle $\alpha$, these paths are modified to 1,2,2′,4,4′,5,$T_2$ and 1,6,7′,8,$T_1$. $T_1$ and $T_2$ enter the telescope in parallel and produce interferences visible in the principal focal plane, provided the rays $T_1$ and $T_2$ are not too far apart, in practice not more than 1 or 2 mm. Interference fringes therefore will always disappear if the angle $\alpha$ is excessive, but the limits are adequately wide for all purposes. The essential constants of the apparatus are to be

$$(9,1) = (6,3) = b; \quad (1,2) = (6,7) = c; \quad (9,3) = (1,6) = 2R,$$

$R$ being the radius of rotation.



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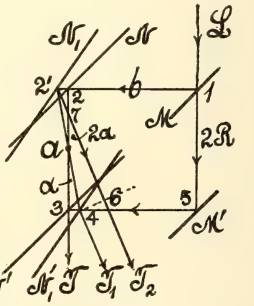


2

When the mirror *mm* is rotated to *m′m′* over the angle $\alpha$, the new upper path will be

$$c + R \tan \alpha + d + e + g,$$

where

$$(2',4) = d, \ (4,4') = e, \ (4',5) = g,$$

the plane (8,5) = $q$ normal to $T_1$ and $T_2$ being the final wave front. The lower path is similarly $2\,R + (c - R \tan \alpha) + d'$ to the same wave front (8,5) where (7′,8) = $d'$. Hence (apart from glass paths which have been treated, the path difference $n\lambda$ ($n$ being the order of interference) should be

$$n\lambda = 2\,R\,(\tan \alpha - 1) + d - d' + e + g.$$

The figure in view of the laws of reflection then gives us in succession

$$\begin{aligned}
d &= (b + c + R \tan \alpha)/(\cos 2\alpha + \sin 2\alpha),\\
d' &= (b + c - R \tan \alpha)/(\cos 2\alpha + \sin 2\alpha),\\
e &= 2\,R/(\cos 2\alpha + \sin 2\alpha),\\
g &= 2\,R \sin 2\alpha\,(1 + \tan \alpha)\,(\cos 2\alpha - \sin 2\alpha)/(\cos 2\alpha + \sin 2\alpha),\\
q &= 2\,R \sin 2\alpha\,(1 + \tan \alpha).
\end{aligned}$$

To obtain $g$ it is sufficient to treat the similar triangles (3,8,9′) and (9,8′,9′) where $h = (9,4)$, $h' = (3,8)$, $k = (9,9')$, $l = (9,8')$ may be found in succession as the normal distance between the mirrors $M$ and $M'$ is $R\sqrt{2}$, so that finally

$$g = (h - l) \sin (45° - 2\alpha), \quad q = (h - l) \cos (45° - 2\alpha).$$

If these quantities are introduced into the above equation for $n\lambda$ we may obtain after some reduction

$$n\lambda = 4 R \sin \alpha (\cos \alpha - \sin \alpha).$$

Since $n\lambda = 2\Delta N \cos i$, $\Delta N$ being the normal displacement of the mirror $M'$, and $i = 45°$, the corresponding equation to the second order of small quantities $\alpha$ is

$$\Delta N/\Delta\alpha = 2 R (\cos \alpha - \sin \alpha)/\cos i = 2\sqrt{2} R (1 - \alpha - \alpha^2/2).$$

If $\alpha$ is sufficiently small, the coefficient is simply $2 R/\cos i$ as used heretofore.

There remain the glass paths which for the rays $d$ and $d'$ are compensated. Additionally the upper ray has a glass path 3 displaced to 4′. The lower ray has the fixed path at 1, and this is equal to the other at 1, since the angles are 45°. Thus the variable part of the glass paths at 3 to 4′ is uncompensated and the angle of incidence changes from 45° to $45° - 2\alpha$. The reflecting sides of the plates are silvered. Hence $e (\sin i - \cos i \tan r) 2\Delta\alpha$ must be added to the equation.

2. *Rotating Doublet.*—The second case, figure 2, in which the auxiliary mirror of the preceding apparatus is omitted is, curiously enough, inherently simpler. $M,M', N,N'$, are mirrors, half silvered at (1) and (3) and the two latter on a vertical axis $A$ and rigidly joined by the rail (2,3). The mirrors being preferably at 45°, the component rays are $1,2,3,T$ and $1,5,3,T$, the mirror $M'$ being on a micrometer with the screw normal to the face. The ray parallelogram is made up as before of $(1,2) = b = (3,5)$ and $(1,5) = 2 R = (2,3)$. When the rail (2,3) is rotated over an angle $\alpha$, the mirrors take the position $N_1$ and $N_1'$ at an angle $\alpha$ to their prior position and the angle of incidence is now $45° - \alpha$. The new paths, if (4,6) is the final wave front, are thus $(1,2,2', 6,T_2)$ and $(1,5,4,T_1)$. The rays $T_1$ and $T_2$ are parallel and interfere in the telescope. Hence the path difference introduced by rotation is ($n$ being the order of interference)

$$n\lambda = b + R \tan \alpha + (2 R/\cos \alpha) \cos \alpha - (2 R + b - R \tan \alpha) = 2 R \tan \alpha,$$

for the triangle $(a,7,2')$ is isosceles and its acute angles each $\alpha$.

The rays $T_1$ and $T_2$ have now separated and the amount (4,6) is also $2 R \tan \alpha$. When this exceeds a few millimeters the interferences vanish.

A correction must however be applied, since in the practical apparatus the mirrors rotate at a fixed distance apart. Hence the mirror $N_1$ must be displaced toward the right (shortening the path) by the normal distance

$$e (R/\cos \alpha - R) \cos 45°$$

and the mirror $N_1'$ toward the left by the same amount. The path difference introduced is thus a decrement and is twice the $2\,e\cos(45° - \alpha)$ of each mirror. Thus the total correction to be subtracted from the equation is after reduction $2\,R\,(1 - \cos\alpha)\,(1 + \tan\alpha)$. Hence the equation becomes ultimately

$$n\lambda = 2\,R\,(\sin\alpha + \cos\alpha - 1).$$

To the second order of small quantities, if $i = 45°$ is the angle of incidence, and $\Delta n$ the normal displacement of $M'$,

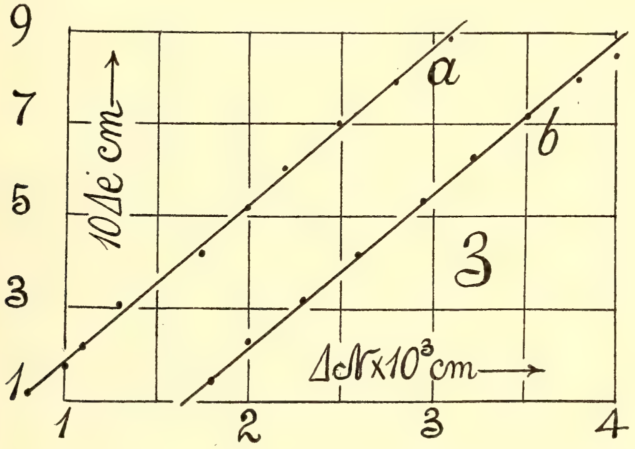
$$\Delta N/\Delta\alpha = R(1 + \Delta\alpha/2)/\cos i.$$

As all the mirrors receive the light on their silvered sides, $M$ originally compensates $N$ if the mirrors are identical in thickness and glass. But the transmission at 3 varies as the angle of incidence changes from $i = 45°$ to $45° - \alpha$. The glass path here decreases by $e\,(\sin i - \cos i\,\tan r)\,\Delta\alpha$, where $e$ is the plate thickness, $r$ the angle of refraction. The path difference as above reckoned has thus been increased by this amount and this quantity is to be added to the right hand member. The effect will not usually exceed a few per cent of the air path difference, and the ratio is the same as above.

3. *Ocular Micrometer.*—It has been stated that the motion of the fringes across the field of the telescope, $T$, is astonishly swift. Hence it is often desirable to insert a micrometer here, as the displacement of fringes can thus be much more accurately and easily measured than at the micrometer along the normal of the opaque mirror, $M'$, of the interferometer. If the latter is of the type using an auxiliary mirror, *mm*, figure 1, the fringes may even be established of a size to correspond with the ocular micrometer, by rotating the auxiliary mirror; but this is not usually necessary. A good ocular plate micrometer was at hand dividing the width of field (about 1 cm.) into 100 parts, the divisions being 0.1 mm. One-tenth of this is easily estimated by the eye in view of the strong eye lens. The light from the collimator at $L$ should completely fill the field, a condition which may be fulfilled by suitably placing the former, modifying its objective. After completing such preliminary adjustments with the fringes, made very sharp and the ocular scale equally so, this is to be placed at right angles to the fringes. Let $\Delta e$ denote their displacement measured in centimeters on the ocular scale and $\Delta N$ (cm.) the displacement of the opaque mirror $M'$ of the interferometer. The question is whether $\Delta e$ and $\Delta N$ are nearly enough proportional quantities for practical purposes. A number of such standardizations were carried out throughout 1 cm. of $\Delta e$, two of which are shown in detail in figure 3. The fluctuation of data is due to air currents across the interferometer. It was not easy to obviate these, and it was not thought necessary for the present purposes. Otherwise the data would have been smooth. There is no doubt that a linear relation may be assumed. In curve *a* the readings of the interferometer micrometer increase, in curve *b* they decrease. If the means be taken from doublets far apart the ratios are

$$(a)\ \Delta N/\Delta e = 0.00310;\ (b)\ \Delta N/\Delta e = 0.00310,$$

and they happen to coincide. Thus $\Delta e$ is 323 times as large as $\Delta N$ and correspondingly easy to measure. The impossibility of setting the micrometer for $\Delta N$ accurately enough, since it is graduated to only $5 \times 10^{-5}$ cm. is completely obviated in $\Delta e$. Moreover, as $2\,\Delta N \cos i = \lambda$ ($i$ being the angle of incidence 45°, and $\lambda$ the mean wave length), we now have $0.0061/\Delta e \cos i = \lambda$; so that the fringe displacement $\Delta e = 0.014$ cm. measured on the ocular micrometer corresponds to the wave length of light in the interferometer measurements. This is more than one scale part. There is however no difficulty in making the fringes larger and obtaining a much more sensitive apparatus in proportion. The achromatic fringes, moreover, when properly produced, contain a distinctive central black line, compatible with the measurement of 0.1 scale part, as here given; i.e. measurement to a few millionths of a centimeter are thus easily feasible under proper surroundings. The apparatus will be used elsewhere.



If $\Delta\varphi$, the angular fringe breadth, is given, $\Delta e/\Delta N$ may be computed from the equation in the earlier paper, to be

$$\Delta e/\Delta N = 2\,L\Delta\varphi \cos i/\lambda$$

or

$$L\Delta\varphi = \lambda/(2 \cos i.\Delta N/\Delta e)$$

as the radius is the length $L = 19.5$ cm. of the telescope. Hence the fringe breadth in centimeters is, if

$$\lambda = 6 \times 10^{-5}\text{cm.}, i = 45°, \text{and } 10^3\Delta N/\Delta e = 3.1,$$
$$L\Delta\varphi = 0.014 \text{ cm.},$$

the value actually observed. Thus if $\Delta\varphi$ is given or measured, $\Delta e/\Delta N$ may be deduced.

The question finally to be determined is thus the value and the meaning of the fringe breadth $\Delta\varphi$. Since

$$2\,\Delta N \cos i = n\lambda$$

if $\Delta N = \Delta N_0$ is constant and also $\lambda$, we may then write

$$\Delta\varphi = di/dn = -\lambda/2\,\Delta N_0 \sin i.$$

Furthermore if $\Delta\theta$ is the angular displacement of fringes

$$\Delta\theta = n\Delta\varphi = -(\Delta N/\Delta N_0) \cot i,$$

if $n\lambda$ is replaced by its value and $\Delta N$ is small compared with $\Delta N_0$. If it is not, since $\Delta N_0$ involves $\Delta N$, we must state the case thus:

$$-\Delta\theta = \cot i \int_{\Delta N_0}^{\Delta N_0 + \Delta N} dN / dN_0,$$

or on integrating and expanding the natural logarithm

$$-\Delta\theta = \cot i\,[\Delta N/\Delta N_0 - (\Delta N/\Delta N_0)^2/2 + \ldots]$$

and $\Delta e = L\Delta\theta$.

In the above measurements

$$\Delta\varphi = L\Delta\varphi/L = 7.2 \times 10^{-4}$$

whence apart from signs

$$\Delta N_0 = \lambda/2\Delta\varphi \sin i = 0.06 \text{ cm., nearly,}$$

whereas the maximum displacement $\Delta N$ throughout the whole series (equivalent to the telescopic field width) does not exceed $\Delta N = 5 \times 10^{-3}$ cm. Hence $(\Delta N/\Delta N_0)^2/2$ may here be neglected to about 1/300 and (again apart from signs) since $i = 45°$,

$$\Delta e = L(\Delta N/\Delta N_0) = 325\ \Delta N,$$

as it should be; i.e., the relation of $\Delta e$ and $\Delta N$ is practically linear, if the displacement $\Delta N$ is not excessive or goes beyond the equivalent of field width.

As the determination of $\Delta N_0$ is inconvenient we thus come back to the practical equation already used, or

$$\Delta N/\Delta\theta = \lambda/2\Delta\varphi \cos i,$$

or if $Ld\varphi = \delta e$ and $\Delta e$ and $\delta e$ are the fringe displacement and the fringe breadth measured on the same ocular micrometer,

$$\Delta N/\Delta e = \lambda/2\ \delta e \cos i,$$

With this deduction the equations of long distance interferometry, etc., form in terms of $\delta e$ the fringe breadth and the fringe displacement $\Delta e$ which may be recorded here, $d$ being the distance,

$$\Delta\alpha = (\lambda/2R\delta e)\Delta e;\ d = (bR\delta e/\lambda)/\Delta e.$$

4. *Collimator Micrometer.*—For many purposes even better conditions are obtainable by replacing the slit of the collimator by a plate glass micrometer. The magnification in such a case is usually greater and since the telescope now contains no fiducial lines, it need not be fixed, but may be shifted

at pleasure so long as the collimator is fixed. This is often a great convenience in working with the achromatic fringes; but I will pass these details over here.[2]

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[1] These PROCEEDINGS, 3, 1917, (563). In this note the glass paths, which are of less importance than the air paths, are too much accentuated.

[2] Abridged from a Report to the Carnegie Institution of Washington, D. C.

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# *THE BRAIN WEIGHT IN RELATION TO THE BODY LENGTH AND ALSO THE PARTITION OF NON-PROTEIN NITROGEN, IN THE BRAIN OF THE GRAY SNAPPER (NEOMAENIS GRISEUS)*

BY SHINKISHI HATAI

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Communicated by A. G. Mayer, December 24, 1917

The predatory fish called the gray snapper, *N. griseus* was mainly used for the present investigation, which was conducted at Tortugas, Florida in the summer of 1917. The following are the more important facts brought out.

*1. The Brain Weight in Relation to Body Length.*—Altogether observations have been made upon 74 brains of the gray snapper. It was found that the relation of brain weight to the increasing body length, from 150 mm. upward, is practically linear and may be satisfactorily expressed as $y = a + bx$, where $y$ represents brain weight in grams and $x$ the body length in millimeters, and $a$ and $b$ are the constants with the values, in this instance, $-0.333$ and $0.00433$ respectively. (Body length is measured from the tip of the snout to the crotch of the tail.)

It is well known that in the adult stage the relation between brain weight and body length or body weight is practically linear, even in the case of some mammals[1] but it is remarkable to find the linear relation in fish, when they are so small. This linear relation during the period of early growth probably means that in the snapper the brain reaches its structural maturity early, and that the subsequent increase in weight indicates merely a uniform swelling of the nervous tissue as a whole.

On account of scantiness of the data for specimens less than 200 mm. in body length, I am unable to present a complete record of the growth of the brain. However, it appears from the general trend of the growth-curve that with the possible exception of the very early period, the relation between the brain weight and body length is linear.

Kellicott[2] studied the growth of the brain in the smooth dogfish in respect to the body weight, and found the graph to resemble that of the mammalian brain (logarithmic curve) and thus to differ strikingly from that for the gray snapper. This difference may be due to the fact that in the dogfish the brain possesses a voluminous cerebellum, as well as olfactory bulbs, and the combined weights of these two structures being greater than that of the rest of the brain, while these two structures in the gray snapper are very small. It appears that these two parts, olfactory bulbs and cerebellum of the dogfish brain, grow very rapidly during the earlier period, thus giving a form of the graph similar to that of the mammal.

2. *Percentage of Water in the Brain.*—Altogether 64 snappers were examined. It was found that so far as the present data are concerned, the percentage of water in the small and large fish is nearly identical within a wide range of body length, and therefore the percentage of water does not vary regularly with the length or size of the fish. Similar relations were observed by Donaldson[3] in the brain of the summer flounder and by Scott[4] in the brain of the smooth dogfish.

It is also interesting to note that the average percentages of water obtained by Donaldson, Scott and myself are nearly the same, being 78.4% (flounder), 78.5% (dogfish) and 78.61% (gray snapper) respectively. It is worthy of note that the values given by these fish are not much different from (slightly above) the percentage of water in the adult mammalian brain.

Since the reduction of the water in the brain is induced by the deposition of the 'myelin substance'[5] we may infer that the process of myelination in the fish brain occurs at a very early period, thus producing slight variation from small to large individuals, as here recorded. I have not however been able to determine the period of rapid reduction of the water content in the brain which must take place in this fish in consequence of the appearance of myelin, because of lack of very young material.

3. *Chemical Analysis of the Brain.*—Altogether 51 dried brains as well as 44 fresh brains were used for the purpose of the chemical analysis. With respect to the nitrogen in total solids, nitrogen in ether-alcohol extract, and the lipoid content, the fish brain closely resembles the stem of the rat brain, but significantly differs from the entire rat brain. This is explained by the fact that the fish brain corresponds essentially to the stem of the mammalian brain, owing to the small growth of the cerebrum and cerebellum. The content of non-protein nitrogen is considerably greater in the fish than in the rat brain. This phenomenon is interesting and I wish to call attention to two factors which may have some bearing on it.

It seems probable that on account of the low grade of organization of the fish brain, the physical consistence of the nervous system may not be as stable as that of more highly organized mammalian nervous systems, and thus the wear and tear process may be much greater, producing a correspondingly greater amount of waste products in the fish brain.

Again it has been found by several investigators that fish blood contains a greater abundance of non-protein nitrogen compared with human blood. The content of non-protein nitrogen in the rat blood is nearly identical with that in human blood.[6] It seems therefore conceivable that the high content of the non-protein nitrogen in the fish blood, accompanied by a slow circulation, might favor the deposition of waste products, and at the same time their slow removal would further tend to increase the accumulation.

[1] Donaldson, H. H., *J. Comp. Neur., Wistar Inst., Philadelphia,* **19,** 1909, (155–167).

[2] Kellicott, W. E., *Amer. J. Anat., Wistar Inst., Philadelphia,* **8,** 1908, (319–353).

[3] Donaldson, H. H., On the percentage of water in the brain of the summer flounder (*Paralichthys dentatus*) according to the body weight. MS. 1905.

[4] Scott, G. G., *Proc. Soc. Exp. Biol. Med., New York,* **9,** 1912.

[5] Donaldson, H. H., *J. Comp. Neur., Wistar Inst., Philadelphia,* **26,** 1916, (443–451).

[6] Hatai, S., *Ibid.*, **28,** 1917, (361–378).

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# *THE ROTATION AND RADIAL VELOCITY OF THE CENTRAL PART OF THE ANDROMEDA NEBULA*

By F. G. Pease

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Communicated by G. E. Hale, December 27, 1917

Indications of rotation in spiral nebulae are quickly obtained by means of spectrographs of very short focus and small dispersion, especially when a wide slit and devices for narrowing the spectrum are used. For definitive values of the rotation, however, it is necessary that the scale be relatively large, the slit narrow, and the exposures very long.

To investigate the rotation of the great nebula in Andromeda, which had been reported by V. M. Slipher at the nineteenth meeting of the American Astronomical Society, an exposure of seventy-nine hours was made during August, September, and October, 1917, with the focal-plane spectrograph of the 60-inch reflector. This instrument which is used at the primary focus of the large mirror has a collimator of 135 mm. focus, a 60° prism of ultra-violet glass, and for the camera a 154 mm. Cooke lens. The dispersion is such that 5.3 mm. corresponds to the interval from λ 4930 to λ 4950. The ratio of focal lengths of camera and collimator is 1.14, and the spectrograph therefore magnifies the image on the slit by this amount.

TABLE 1

| DISTANCE FROM NUCLEUS | | NUMBER OF SECTIONS | ADAMS | MISS BURWELL | MEAN |
|---|---|---|---|---|---|
| | *seconds* | | *km.* | *km.* | *km.* |
| South Prec. | 162 | 1 | (−355) | (−469) | (−412) |
| | 121 | 2 | 369 | 400 | 385 |
| | 66 | 2 | 342 | 342 | 342 |
| | 25 | 1 | 310 | 312 | 311 |
| | 0 | 1 | 307 | 317 | 312 |
| North Foll. | 41 | 2 | 288 | 308 | 298 |
| | 96 | 2 | 278 | 288 | 283 |
| | 137 | 1 | (292) | (257) | (274) |

During the exposure the slit crossed the nucleus and coincided with the major axis which lies in position angle 40°. The slit-width was 0.025 mm., and the spectrogram was traversed longitudinally by comparison spectra at intervals of 1 mm. (see fig. 1).

The results of measures by Mr. Adams and Miss Burwell are given in table 1. The correction for the earth's motion is slight and has been neglected. To reduce the accidental errors and equalize the weights somewhat, results for adjacent sections of spectra in the outer regions have been combined. The values for extreme sections, which are very uncertain, are given separately and appear in parentheses in the table. The values of the distance from the nucleus have been corrected for magnification by the spectrograph. The tabulated radial velocities must be corrected for the inclination of the nebula to the line of sight. Assuming the nebula to be circular, the ratio of the major and minor axes of its apparent elliptical outline, which is 4 to 1, is a measure of the inclination. We thus find approximately 14° as the inclination, and the velocities must accordingly be multiplied by 1.03 to reduce them to the plane of the nebula.

The mean velocities of table 1 are shown in figure 2, the ordinates being kilometers per second and the abscissae seconds of arc. The velocity curve

$$y = -0.48\,x - 316$$

was determined graphically. From this equation we find:

1. The radial velocity of the nebula is −316 km., a value in good agreement with the earlier measures:

| | km. |
|---|---|
| Pease and Adams[1] | −329 |
| Pease and Adams[2] | −297 |
| Slipher[3] | −300 |
| Wolf[3] | −350 |
| Wright[3] | −304 |

2. The linear velocity of rotation at a point 2 minutes of arc from the nucleus is 58 per second.

3. Within the distance measured and the limits of accuracy of the measures the change of rotational velocity with distance seems to be linear, although there may be variations at individual points in the nebula.

4. Whether the motion of the nebula is inward or outward along the arms of the spiral depends upon the inclination of the nebula.

From a spectrogram of eighty-four hours exposure made at intervals during August, September, and October, 1916, with the slit placed on the minor axis of the nebula, Mr. Adams obtains the results shown in table 2.

These velocities, which are nearly constant, are also shown in figure 2 the velocity curve obtained for points along the major axis, and greatly with

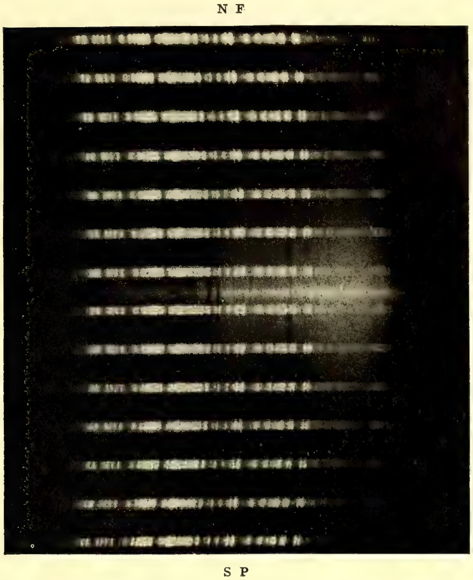

FIG. 1. SPECTRUM OF THE CENTRAL PARTS OF THE ANDROMEDA NEBULA. EXPOSURE 79 HOURS

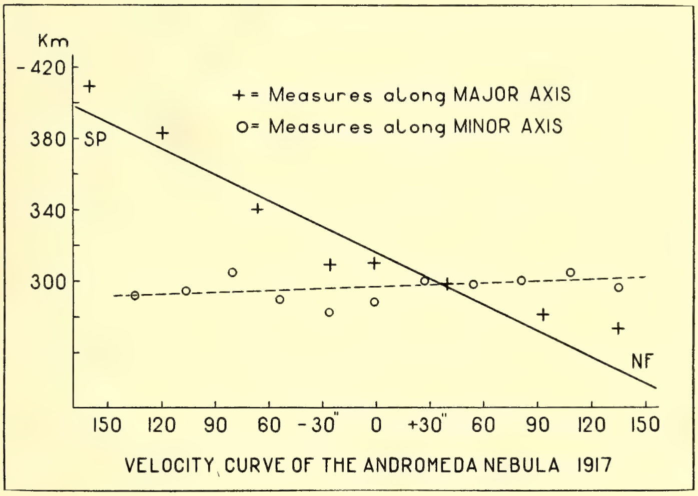

FIG 2

strengthen the conclusions derived from the latter by showing that the variable velocity given by the spectrogram first described is without doubt to be attributed to the rotation of the nebula.

The arms of the cross in figure 3 indicate the length and position of the slit on the nebula.

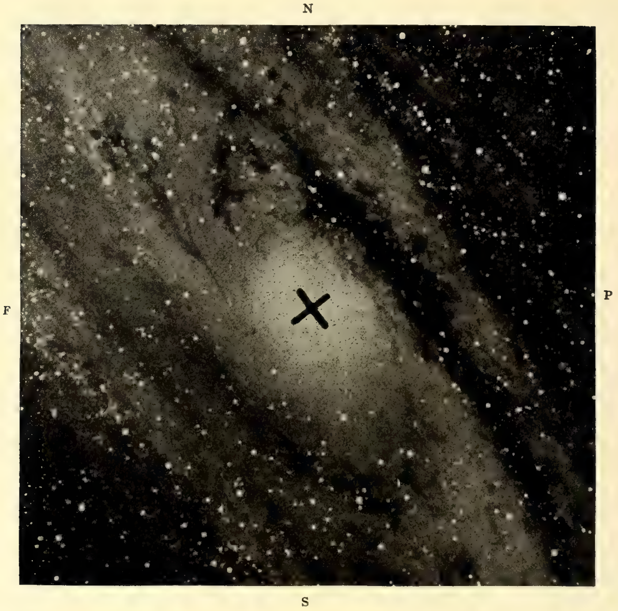

FIG. 3. DIAGRAM SHOWING THE LENGTH AND POSITIONS OF SLIT ON THE NEBULA

TABLE 2

| SECTION OF SPECTRUM | RADIAL VELOCITY | NUMBER OF LINES |
|---|---|---|
| | *km.* | |
| 5th, below | −293 | 3 |
| 4th | 297 | 4 |
| 3rd | 306 | 7 |
| 2nd | 291 | 5 |
| 1st | 284 | 6 |
| Nucleus | 291 | 9 |
| 1st, above | 300 | 8 |
| 2nd | 299 | 6 |
| 3rd | 302 | 6 |
| 4th | 306 | 4 |
| 5th | −297 | 5 |
| Mean | −297 | |

Both spectrograms show a solar-type spectrum entirely free from bright lines. Mr. W. P. Hoge assisted in the guiding during the long exposures.

---

[1] Pease, F. G., *Pub. Astr. Soc. Pac., San Francisco, Cal.*, **27**, 1915, (134).

[2] This paper following.

[3] Slipher, V. M., *Pop. Astr., Northfield, Minn.*, **23**, 1915, (21–24), page 23.

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# *THE HEAT CAPACITY OF ELECTRO-POSITIVE METALS AND THE THERMAL ENERGY OF FREE ELECTRONS*

By Gilbert N. Lewis, E. D. Eastman and W. H. Rodebush

Chemical Laboratory, University of California

Communicated December 26, 1917

According to the classical principle known as the equipartition of energy the kinetic energy of every kind of atom which is capable of motion in any direction is the same, and the change in this kinetic energy for a change in temperature of one degree is $3R/2$ calories per gram atom. If in a solid substance each atom instead of moving freely vibrates about a more or less fixed position according to the laws of a simple oscillator, it will possess on the average a potential energy equal to its kinetic energy, and, if we consider the simple case in which the volume is kept constant the heat capacity per gram atom should be equal to $3R$, or 5.97 calories per degree.

According to the empirical law of Dulong and Petit, the atomic heat capacity at constant pressure is the same for most solid elements and equal to a little over 6 calories per degree. Lewis[1] calculated by means of a thermodynamic equation the difference between $C_p$ and $C_v$ and found for a considerable number of elements that $C_v$ is more nearly constant and that its average value at 20°C., when a few elements of low atomic weight are omitted, is 5.9, which is almost identical with the theoretical value.

Much recent work, both experimental and theoretical, has shown that the earlier formulation of the equipartition law can no longer be considered as universally valid but rather as the expression of a limiting law which approaches complete validity the higher the temperature, the heavier the atom and the weaker the constraints operating upon the atom.[2] Every factor which causes a deviation from the equipartition law leads to a smaller specific heat than the one calculated by that principle. If therefore we find for any substance a value of $C_v$ greater than $3R$ per gram atom we are led immediately to consider the possibility that in addition to the atoms other particles, either inside or outside of the atoms, are acquiring thermal energy.[3]

In the calculations of Lewis to which we have referred the value of $C_v$ was found to be 6.4 for sodium and 6.5 for potassium, but the difference between these values and the theoretical values might at that time have been ascribed to experimental error. More recently Dewar[4] has determined the mean atomic heats of a large number of elements between the boiling points of hydrogen and nitrogen. He found that at these low temperatures, where for most elements the atomic heat is only a small fraction of $3R$, that of cesium is 6.3. This is the value of $C_p$, but it seems likely that at such a low temperature the difference between $C_p$ and $C_v$ is very small even for an element like cesium. These facts led us to suspect that an abnormally high value of the atomic heat capacity might be a characteristic property of highly electro-positive metals. We were therefore interested to find that the experiments of Nernst and Schwers[5] on the specific heat of magnesium showed for this metal a change of heat capacity with the temperature which at low temperatures corresponds with that obtained for other monatomic elements,[6] but which at higher temperatures appears abnormally large.

In order to test the supposition that all electro-positive metals would exhibit this kind of abnormality we have determined over a wide range of temperature the specific heats of two metals of the first group, sodium and potassium, and of two metals of the second group, magnesium and calcium. The detailed measurements and the experimental method will be described shortly in the *Journal of the American Chemical Society*. Here we shall give only the general results of the investigation.

Before discussing the heat capacities found for the four metals it will be well to review briefly previous work on the heat capacities of other typical metals. Of these aluminum, copper, zinc, silver, mercury (solid), thallium and lead have been fully investigated over a wide range of temperature. For all of these metals $C_v$ has been found to be the same function of $T/\theta$, where $T$ is the absolute temperature and $\theta$ is the temperature at which $C_v$ is equal to $3R/2$. In other words, if we follow the procedure of Lewis and Gibson and plot $C_v$ against $\log (T/\theta)$ the individual values of $C_v$ for all these metals fall upon the same curve, which they speak of as the regular curve, or the curve of Class I. This may be expressed in still another way by the statement that if the values of $C_v$ as ordinates are plotted against the values of $\log T$ as abscissae the curves for the several elements may be made to coincide by horizontal displacement.

The form of the regular curve is shown by the continuous curve of figure 1, where $C_v$ is plotted against the common logarithm of the temperature, and the individual points are those found for magnesiun. The five lowest points are taken from the measurements of Nernst and Schwers[7] and the remainder represent our own measurements. It is evident that at low temperatures the specific heat curve for magnesium coincides with the regular curve, but that at higher temperatures the specific heat rises more rapidly than in the case of any of the other metals to which we have referred.

That this same phenomenon is exhibited by potassium, sodium and calcium is shown in figure 2, where the regular curve which would fit the experimental points at lower temperatures is drawn as a continuous curve and the individual points represent our experimental determinations of the heat capacity. In each of these cases the heat capacity rises well above the theoretical value $3R$.

The deviation from the regular curve at any one temperature is greater for calcium than for magnesium and greater for potassium than for sodium. We may therefore predict that this phenomenon is more marked the more electropositive the metal, but that it will be found for every substance at high temperatures. According to modern theories of atomic structure the atom can no longer be regarded as the ultimate unit of matter. Besides the atomic nucleus there are electrons, and all of these particles, if capable in any degree of independent motion, should according to the earlier theory of the equipartition of energy, acquire their full share of thermal energy. If, however, we admit that at a given temperature deviations from the equipartition principle occur to an extent which is greater the lighter the particle and the more rigid the constraint which holds it to a fixed position, then it follows that at a temperature at which other particles are gaining thermal energy approximately according to the equipartition law, the electron which is by far the lightest of all particles, will acquire very little energy unless bound by very weak con-

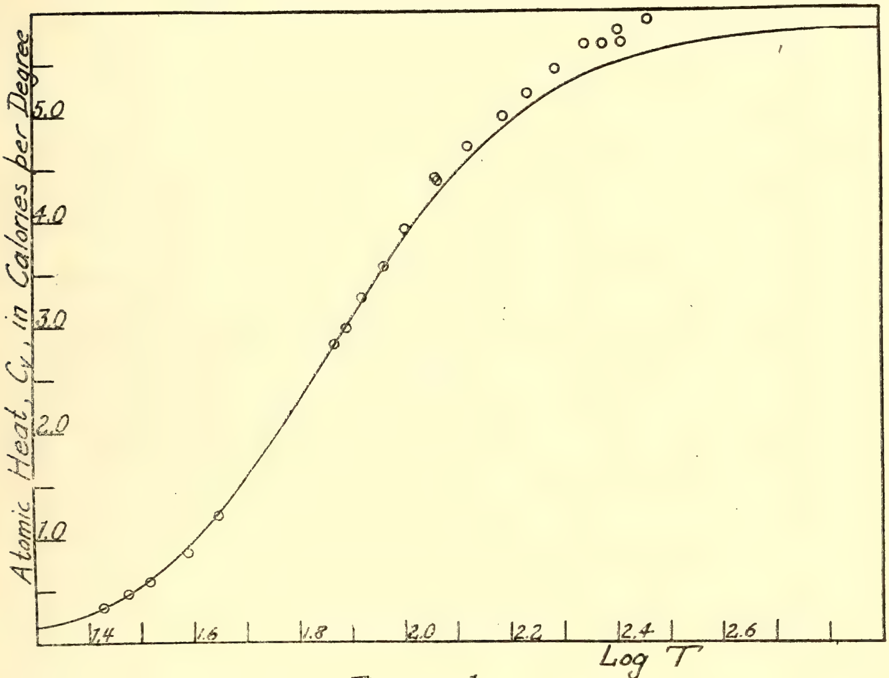

Figure 1.

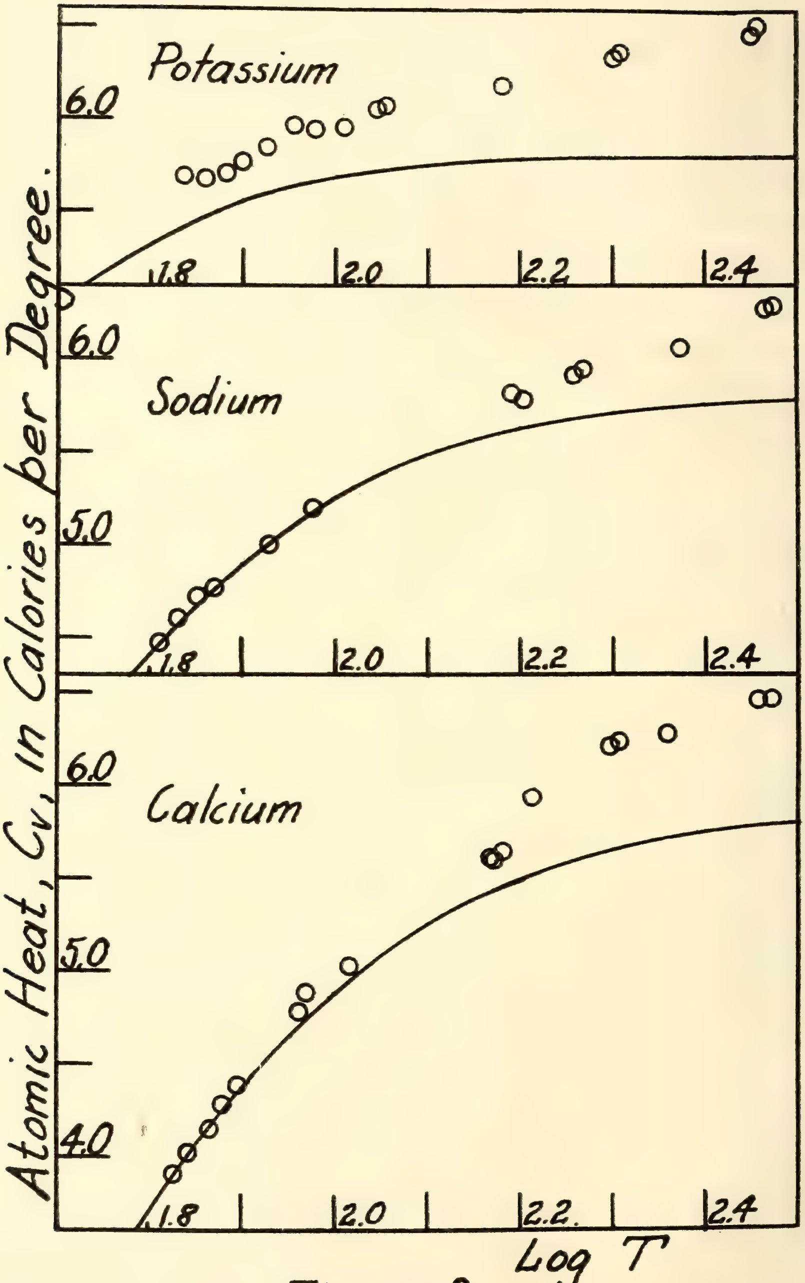
Potassium
Sodium
Calcium
6.0
6.0
5.0
6.0
5.0
4.0
1.8
2.0
2.2
2.4
Atomic Heat, Cv, in Calories per Degree.
Log T

Figure 2.

straints. The optical properties of most substances show their electrons to be held rather rigidly, but many of the properties of metals, and especially of metals pronouncedly electro-positive in character, indicate a high degree of electronic freedom.

It is therefore our belief that in the metals under consideration the difference between the heat capacity observed and that calculated from the regular curve, which fits the experimental curve at low temperatures, may be regarded as representing the actual heat capacity of their more loosely bound electrons. Whether these electrons are 'free' in the sense that each electron occupies a position symmetrical with respect to two or more atoms, or whether they remain attached to individual atoms, we should expect them to add to the heat capacity of the substance, provided that they are held by sufficiently weak constraints.

We have thus an entirely new method of investigating the freedom of electrons in a metal, and it is to be hoped that when further quantitative data are available a comparison of the results obtained by this method with those obtained through a study of the photo-electric effect, or the Volta effect, will prove of interest.

[1] Lewis, *J. Amer. Chem. Soc., Easton, Pa.* **29**, 1907, (1165); *Zs. anorg. Chem., Hamburg,* **55**, 1907, (200).

[2] Lewis and Adams, *Physic. Rev., Ithaca, N. Y.*, (Ser. 2), **4**, 1914, (331).

[3] Langmuir, *J. Amer. Chem. Soc., Easton, Pa.*, **38**, 1916, (2236), calls attention to the possibility that the atom in a solid may not be vibrating in simple harmonic motion and hence that the potential energy may not be equal to the kinetic energy, as has been assumed. However, if this were the case the potential energy would in all probability be less than the kinetic energy and not greater.

[4] Dewar, *London, Proc. R. Soc., A*, **89**, 158, 1913, (158).

[5] Nernst and Schwers, *Berlin Sitz. Ber. Ak. Wiss.*, 1914, (1), (255).

[6] See Lewis and Gibson, *J. Amer. Chem. Soc., Easton, Pa.*, **39**, 1917, (2554).

[7] The four highest points of Nernst and Schwers have been omitted since they appear to us to be erroneous.

---

## THERMO-ELECTRIC DIAGRAMS ON THE P-V-PLANE

By Edwin H. Hall

Jefferson Physical Laboratory, Harvard University

Read before the Academy, November 21, 1917

About thirteen years ago I made an ill-directed and unfortunate attempt to analyze the electromotive force of a thermoelectric circuit, that is, to identify and explain the local e.m.f's which, taken together, give the e.m.f. of the circuit as a whole. I am now making a new attempt. I know that my present effort is more intelligently directed than the former one, and hope that it will prove to be more fortunate; but I do not claim for it finality.

I shall in the present paper discuss the matter on the assumption that the 'free' electrons within the metals are the only ones moving progressively in the maintenance of a current and the only ones taking part in thermo-electric action. For the sake of generality I shall not assume that these electrons have the same kinetic energy as gas molecules or even have the same kinetic energy in different metals at the same temperature. In fact, I shall in using the equation

$$pv = GRT, \qquad (1)$$

where $v$ is the volume of a gram of free electrons within a metal and $G$ is the number of electrons in this gram, assume that $R$ may be a function of $T$ and may vary from metal to metal. I shall follow common practice in supposing $n$, the number of free electrons per unit volume, to be greater in some metals than in others and to increase in all metals with rise of temperature. I shall omit, as being unnecessary for a general explanation of thermo-electric action, the assumption of a specific attraction of metals for electrons.

In a detached bar of metal $\alpha$, kept hot at one end and cold at the other, there is a higher free-electron pressure at the hot end than at the cold end, partly because of the greater value of $n$, partly because of the higher average heat energy of the electrons, at the hot end. Hence there is a mechanical tendency of the free electrons to move toward the cold end of the bar. But it cannot be said with confidence that this tendency, if unrestricted, would result in a condition of equal pressure from end to end of the bar; for it is quite possible, as I have pointed out in a previous paper,[1] that it would produce the condition of equilibrium which holds in 'thermal effusion.'

I shall make use in turn of two alternative hypothesis;

($A$) That the mechanical tendency is toward equality of pressure, represented by the equation

$$nRT = a \text{ constant},$$

from end to end of the bar.

($B$) That the mechanical tendency is toward thermal effusion equilibrium, represented by the equation

$$n\,(RT)^{\frac{1}{2}} = a \text{ constant},$$

from end to end of the bar.

Under neither hypothesis will there be any sufficient movement of electrons to produce an appreciable alteration of $n$ from its natural value at any point,—that is, the value it would have at this point if the whole bar were at the temperature of this point. For the free electrons are mingled, naturally, with an equal number of positive metal ions which are not free to move through the bar, and therefore these electrons are powerfully restrained from any large movement down the pressure gradient. The proper distribution of an exceedingly small excess of them along the surface of the colder half of the bar, with a corresponding deficiency in the warmer part, is enough to balance the mechanical-pressure tendency and maintain equilibrium. Of course this equilibrium is a mobile one, individual electrons moving in either direction through any cross-section of the bar constantly, but with zero net result so long as the bar is isolated and of unchanging temperature-gradient.

The pressure gradient, and the balancing electric potential, will depend on the rate at which $n$ increases with increase of $T$ in this metal $\alpha$. Let this rate be such that the changes of pressure and volume suffered by one gram of electrons in passing, under conditions differing only slightly from those of equilibrium, from temperature $T_1$ to temperature $T_2$, or *vice versa*, through the metal $\alpha$ are represented by the curve[2] $A\ D$ in figure 1.

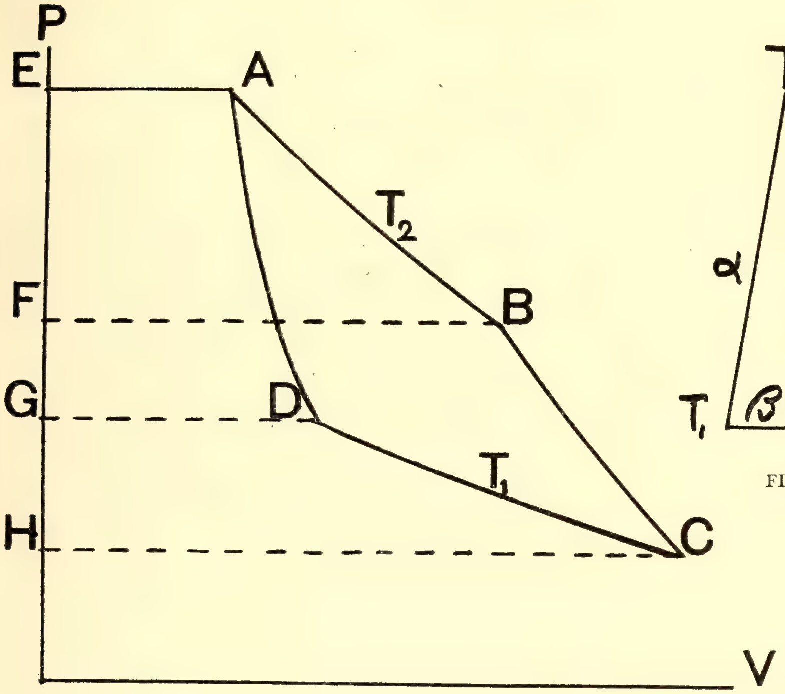

FIG. 1

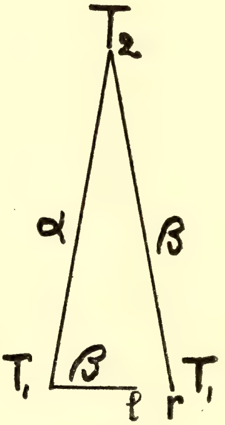

FIG. 2

If this curve is steeper than the adiabatic curve of the moving body of electrons, this body will take up heat from the metal during its progress from $T_1$ to $T_2$; that is, the Thomson heat-effect in this metal will be of the same sign as that in iron. If the line is less steep than the adiabatic, the Thomson heat-effect will be of the same sign as that in copper. In lead the adiabatic line would be followed very nearly.[3]

Let the line $B\ C$ be for the metal $\beta$ what the line $A\ D$ is for $\alpha$.

*Under hypothesis A.*—In order to have electrical equilibrium within the bar, we must have

$$dp = f\,n\,dl \tag{2}$$

where $f$ is the electrostatic force acting on a single electron in the direction of increasing T, and $dl$ is the element of length of the bar corresponding to the change of pressure $dp$. Hence the decrease of negative electrostatic potential from the cold to the hot end of the bar is

$$P_1 - P_2 = \frac{1}{e}\int_0^l f dl = \frac{1}{e}\int_0^l \frac{fndl}{n} = \frac{1}{e}\int_0^l \frac{dp}{n} = \frac{1}{Ge}\int_{p_1}^{p_2} v\,dp, \tag{3}$$

where $e$ is the electron charge, $v$ is the volume of 1 gram of the electrons, and $G$ is the number of electrons therein. The value of the last integral is evidently equal to the area $E\,A\,D\,G$ for the bar $\alpha$ and to the area $F\,B\,C\,H$ for the bar $\beta$.

Each of the lines $A\,D$ and $B\,C$ corresponds to a state of equilibrium for its metal. Let us now suppose the two metals to be joined together at the hot end while an additional piece of the metal $\beta$, at temperature $T_1$, is joined to the $T_1$ end of $\alpha$, as in figure 2, leaving a gap between two pieces of $\beta$, with temperature $T_1$ at both $l$ and $r$. We may, if it seems desirable, instead of joining the two metals directly, insert at each junction a bar, or bridge, of alloy changing in composition from pure $\alpha$ at the left end to pure $\beta$ at the right end, the whole length being at the temperature of the $\alpha$ and $\beta$ adjacent. See Figure 3. Through each of the bridges we shall have a gradual change of $n$, from the valuable proper to $\alpha$ to the value proper to $\beta$.

At the instant when the junctions indicated are effected there will probably be a slight movement of electrons from one metal to the other, lowering a little the electric potential of one bar as a whole and raising that of the other; but the gradient of mechanical pressure, and so the balancing gradient of electric potential, will remain sensibly unaltered in each. Along each of the isothermal bridges of alloy the general law of equilibrium will be the same as that which we have seen to hold in each of the homogeneous but now isothermal bars; that is, $dp$ equals $fndl$. The volume-pressure changes through the bridge at $T_1$ will be those shown by the isothermal line $A\,B$ of figure 1; the corresponding changes through the bridge at $T_1$ will be those shown by the isothermal $D\,C$. The increase of negative electric potential through the bridge at $T_2$ from $\alpha$ to $\beta$ will be, according to equation (3), represented by $\frac{1}{Ge} \times$ (*the area E A B F*). The corresponding increase at the $T_1$ junction will be $\frac{1}{Ge} \times$ (*the area G D C H*).

We still have equilibrium, the circuit being open at $lr$. But is the potential of the $\beta$ at $l$ equal to that of the $\beta$ at $r$? We can answer this question readily. Starting in $\alpha$ (fig. 3), close to the $T_2$ junction, going across the bridge and then down $\beta$ to $r$, we have as the gain of negative potential

$$P_r - P_{\alpha_2} = \frac{1}{Ge}(EABF + FBCH) = \frac{1}{Ge} \times EABCH \qquad (4)$$

Starting again at the same point as before and going down $\alpha$, then across the lower bridge to $\beta$, we get

$$P_l - P_{\alpha_2} = \frac{1}{Ge}(EADG + GDCH) = \frac{1}{Ge} \times EADCH. \qquad (5)$$

We have, then, by subtraction

$$P_r - P_l = \frac{1}{Ge} \times ABCD. \qquad (6)$$

If now we choose to connect $l$ and $r$ by means of some very large resistance, the current will flow from $r$ to $l$ through this resistance, while the conditions within $\alpha$ and $\beta$ will remain almost unchanged. We shall have a thermo-electric current passing clockwise around the circuit of Fig. 2, or Fig. 3, and the e.m.f. maintaining this current will be $\frac{1}{Ge} \times A\,B\,C\,D$. The amount of electrical work done by a gram of electrons, $Ge$ units, in passing once around this circuit is $A\,B\,C\,D$, which is, of course, equal to the mechanical work of the pressure-volume cycle through which the gram of electrons passes.

*Under Hypothesis B.*—In this case, as I have elsewhere shown,[1] we have, as the condition of electrical equilibrium in a detached bar with a temperature gradient,

$$dp = \tfrac{1}{2}\,p\left(\frac{dR}{R} + \frac{dT}{T}\right) + fndl. \qquad (7)$$

Hence

$$P_1 - P_2 = \frac{1}{e}\int_0^l fdl = \frac{1}{e}\int_0^l \frac{fndl}{n} = \frac{1}{e}\int_{p_1}^{p_2} \frac{dp}{n} - \frac{1}{2e}\int_1^2 \frac{p}{n}\left(\frac{dR}{R} + \frac{dT}{T}\right).$$

But

$$\frac{p}{n} = RT = \frac{pv}{G},$$

and so

$$P_1 - P_2 = \frac{1}{Ge}\int_{p_1}^{p_2} vdp + \frac{1}{2Ge}\left(p_1v_1 - p_2v_2\right). \qquad (8)$$

Graphically interpreted, equation (8) gives for the line $AD$, of figure 1 and figure 4, $P_D - P_A = \frac{1}{Ge} \times E'ADD'd'e'$, where $E'$ is a point halfway between $E$ and $A$, while $D'$ is halfway between $G$ and $D$.

For the potential difference of each of the lines $A\,B$, $B\,C$, and $D\,C$, of figure 1, we have a corresponding representation; but these areas all combined, the sum of the areas for $A\,D$ and $D\,C$ being subtracted from the sum of the areas for $A\,B$ and $B\,C$, will give, as before, $\frac{1}{Ge} \times A\,B\,C\,D$, as the net e.m.f. of the circuit.

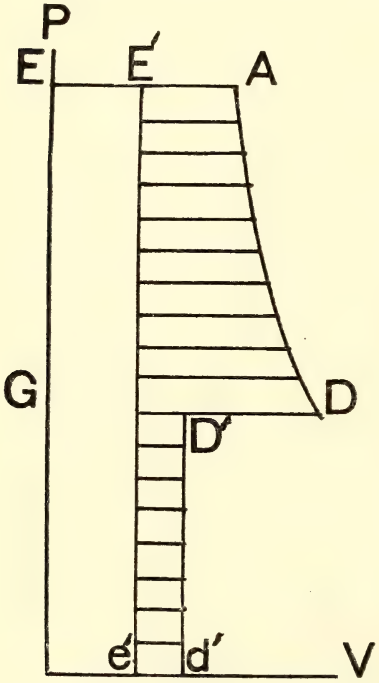

FIG. 4

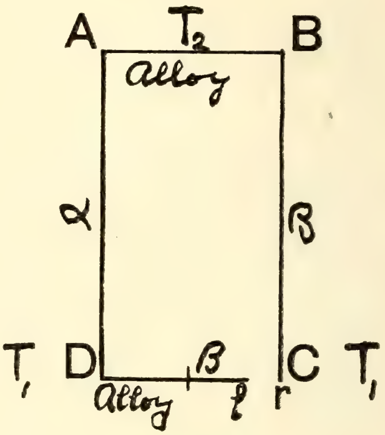

FIG. 3

If the diagram $A\,B\,C\,D$ represented the operation of an ordinary fluid, like air or steam, working in a cylinder under a piston, the path $A\,B\,C$ would represent that part of the cycle in which the fluid, expanding, does work on the piston at the expense of heat energy, while the path $C\,B\,A$ would represent that part of the cycle in which the returning piston does work in compressing the fluid. In the thermo-electric case, under the conditions which we have assumed, the path $A\,B\,C$ represents that part of the cycle in which the expanding electric fluid does work in storing up electric potential energy at the expense of heat energy, while the path $C\,D\,A$ represents that part of the cycle in which electric potential energy works to compress the electric fluid.

So far as mere expansions and contractions are concerned, we may ignore the electrical property of the circulating fluid; for the mutual electrical self-repulsion of its particles is balanced by the action of the equally numerous positive ions.

It is to be noted that no necessity has appeared in this discussion for any 'specific attraction' of either metal for electrons. Ordinary electro-static attraction and repulsion, together with the mechanical, expansive, heat-pressure of the electrons, is apparently enough to account for all that is known to occur in thermo-electric action confined to a metallic circuit. All the evidence we have to show the existence of the so-called 'specific,' or 'essential,' attraction comes from other regions of phenomena, especially from studies of thermionic emission and the Volta contact potential-difference. If such an attraction exists and is active within metals, we need not change the discussion above given except in the interpretation of $P$. This has here been taken as ordinary electrostatic potential. If there is an 'essential attraction' between electrons and matter having no electric charge, $P$ will appear in the formulas just as it appears now, but it will be interpreted as what I have called in a previous paper[1] *virtual potential,* potential due to all attractions and repulsions acting on the progressive electrons.

It is of interest to observe that the e.m.f. along any part of the circuit does not, under either hypothesis (A) or hypothesis (B), necessarily correspond to the amount of heat absorbed in this part. For example, the Thomson heat absorbed along the line $A\ D$ may be positive, negative, or zero, according to the inclination of the line, while the e.m.f. of this part, represented by the area $E\ A\ D\ G$, remains always of the same sign.

The preceding discussion expressly assumes that the free electrons are the only ones which move through a metal. This is a provisional assumption only. If the associated electrons also move progressively, as I have in certain previous papers supposed them to do, the conditions of equilibrium in a detached metal bar, having a temperature-gradient, are different from those indicated in this paper. This matter I hope to discuss at another time.

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[1] Boston, *Proc. Amer. Acad. Arts Sci.*, **50,** 4 July, 1914.

[2] Great steepness of this curve does not, as one might at first suppose, indicate a very rapid increase of $n$ with increase of $T$, but the contrary.

[3] That is, if the moving body of electrons that constitute an electric current obeyed the laws of a perfect manotomic gas, we should have in lead $n \propto T^{1.5}$, very nearly.

# A DETERMINATION OF THE SOLAR MOTION AND THE STREAM MOTION BASED ON RADIAL VELOCITIES AND ABSOLUTE MAGNITUDES

By Gustaf Strömberg

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Communicated by W. S. Adams, December 14, 1917. Read before the Academy, November 21, 1917

In previous determinations of the motion of the sun in space and of the star streams the stars have been divided for discussion according to spectral type or apparent magnitude. The recent investigations of Adams and Strömberg[1] have shown that the intrinsically faint stars have a higher average radial velocity than those that are intrinsically brighter, or in other words, that radial velocity is a function of absolute magnitude. Accordingly an investigation of the solar motion and the stream motion based upon a division of stars into groups of nearly equal absolute magnitude is of exceptional interest, since the dispersion of the radial velocities within each group is considerably less than in the usual case. A brief account of such an investigation is given in this communication.

Of the 1300 stars of the spectral types F, G and K with measured radial velocities which have been used in the discussion, about 700 have absolute magnitudes determined spectroscopically by Adams. For the remainder, observed mainly at the Lick and Mills Observatories, mean parallaxes have been computed by the aid of the following formula connecting proper motion and apparent magnitude:

$$\mathrm{Log}\,\pi = \log A + \log(\mu + c) + m \log \epsilon,$$

in which $\pi$ is the mean parallax, $\mu$ the proper motion, $m$ the apparent magnitude, and $A$, $c$ and $\epsilon$ constants determined by means of the spectroscopic parallaxes. The formula differs from that of Kapteyn,[2] which for later type stars of very small proper motion gives parallaxes that are too small, in the addition of the constant $c$.

*Solar Motion.*—The constants of the solar motion have been determined by a least squares solution of equations of condition of the form

$$V = x_0 \cos\alpha \cos\delta + y_0 \sin\alpha \cos\delta + z_0 \sin\delta + K,$$

in which $V$ is the radial velocity, $-x_0$, $-y_0$, and $-z_0$ the rectangular components of the sun's motion, and $K$ Campbell's $K$-term, the quantity which must be subtracted from each of the radial velocities, corrected for the sun's motion, in order to make their sum equal to zero. The results indicate that the $K$-term has a small positive value in the case of the very luminous stars, and probably a negative value in the case of the fainter stars. For $K$ equal to zero the constants of the solar motion have the values given in table 1. $\overline{M}$ and $\overline{m}$ are the arithmetical means of the absolute and apparent magnitudes, $A$ and $D$ the right ascension and declination of the sun's apex, $V_0$ the sun's velocity in space, and $\vartheta$ the arithmetical mean of the radial velocities corrected for the sun's motion, taken regardless of sign. The results given by the groups of faintest magnitude are necessarily uncertain since the stars included within them are almost exclusively in the northern hemisphere and the distribution is relatively unfavorable.

TABLE 1

CONSTANTS OF SOLAR MOTION ($K = 0$)

| NO. | $\overline{M}$ | $\overline{m}$ | $A$ | $D$ | $V_0$ | $\vartheta$ |
|---|---|---|---|---|---|---|
| *F and G type stars* | | | | | | |
| | | | | | *km.* | *km.* |
| 211 | 0.31 | 4.68 | 251°.4 | +22°.5 | 19.4 | 11.3 |
| 177 | 1.44 | 5.42 | 267.5 | +36.3 | 16.6 | 14.6 |
| 167 | 2.76 | 5.27 | 272.1 | +36.4 | 22.8 | 16.3 |
| 170 | 5.29 | 6.41 | (279.6) | (+10.9) | (27.1) | (23.9) |
| 725 | 2.32 | 5.40 | 268.3 | +26.1 | 20.1 | 17.2 |
| *K type stars* | | | | | | |
| 122 | 0.54 | 4.22 | 279°.6 | +33°.8 | 24.0 | 13.7 |
| 245 | 1.41 | 4.86 | 268.1 | +37.2 | 20.4 | 16.6 |
| 99 | 2.58 | 5.12 | 284.5 | +20.1 | 26.0 | 18.6 |
| 79 | 7.07 | 7.41 | (289.0) | (+26.5) | (22.1) | (26.2) |
| 545 | 2.25 | 5.13 | 277.0 | +32.5 | 22.2 | 18.5 |
| *M type giants* | | | | | | |
| 135 | 1.5 | 4.98 | 264°.2 | +26°.1 | 26.8 | 16.9 |

*All stars of late type*

$A = 270°.9 \pm 3°.3$ $D = +\ 29°.2 \pm 3°.4$ $V_0 = 21.48 \pm 1.02$ km
$\vartheta = 17.7$ km $K = +\ 0.36 \pm 0.60$ km

There appears to be a tendency toward smaller values of the declination for the intrinsically faint stars. If real this effect may be explained on the assumption of a variation in the proportion of stars belonging to the two star drifts. A marked feature of the results is the increase of the average radial velocity with decreasing brightness.

*Stream Motion.*—For a study of stream motion the stars with measured radial velocities have been divided into three groups, as follows, according to absolute magnitude:

| GROUP | NO. | $\overline{M}$ | $\overline{m}$ | $\pi$ | $\vartheta$ |
|---|---|---|---|---|---|
| | | | | | km. |
| I | 509 | 0.76 | 4.83 | 0″.015 | 13.11 |
| II | 513 | 2.08 | 5.09 | 0.025 | 17.13 |
| III | 260 | 6.05 | 6.82 | 0.070 | 25.88 |

Each group contains stars of spectral types F, G and K, the third group, comprising the faintest stars, also including 11 stars of the dwarf M type. The symbol $\pi$ denotes the geometrical mean of the parallaxes, which is readily derived by the formula

$$5 \log \pi = M - m - 5.$$

In accordance with the method of Charlier, the sky has been divided into 48 equal areas situated symmetrically with reference to the galactic equator. If star streaming is studied on the basis of the ellipsoidal or the two drift theory, opposite areas may be combined by assuming that the stream motion is the same in all parts of the space.

Since the sun, however, is not situated at the center of the stellar system, but at a distance from it probably comparable with the mean distance of the later type stars used in this investigation, we might, according to the theory of Turner[3] and Eddington,[4] conceive the stream-motion to be due to the general attraction of the stellar system. It would then differ from point to point and be related to the position of the center and the central plane of the stellar system. Such a theory is supported by Kapteyn's[5] suggestion that there is an acceleration of the first stream; i.e., that the velocity of the first stream is different in different parts of the space.

In order to test for the existence of such a varying stream-motion, I have tried to express the average radial velocity as a rational integral function of the direction-cosines of the line of sight such that the radius vector of the surface thus defined equals the average radial velocity. Using only terms of second order we can in this way determine the stream-motion, if the latter is constant (Eddington[6]). If the stream motion is variable this must be marked by the existence of asymmetrical (odd) terms in the analytical representation of the surface.

In this investigation terms to the third order inclusive have been determined.

If terms of the second order alone are included, opposite areas in the sky may be combined. The resulting directions of the axes of maximum radial velocity $\theta$ are thus:

| GROUP I | | | GROUP II | | | GROUP III | | |
|---|---|---|---|---|---|---|---|---|
| $\vartheta$ | $\alpha$ | $\delta$ | $\vartheta$ | $\alpha$ | $\delta$ | $\vartheta$ | $\alpha$ | $\delta$ |
| km. | | | km. | | | km. | | |
| 16.14 | 98° | +5° | 20.98 | 86° | +10° | 32.4 | 100° | +34° |

The position found for the axes of preferential motion is in good agreement with other determinations which, according to the summary by Eddington, give $\alpha = 94°$, $\delta = +12°$.

The asymmetrical terms, or those of odd order, are next determined separately for the three groups. If those which depend only on the galactic latitude are considered, we obtain expressions for $\theta$ from which the following maximum values may be derived:

| GROUP I | | GROUP II | | GROUP III | |
|---|---|---|---|---|---|
| $\vartheta$ | $b$ | $\vartheta$ | $b$ | $\vartheta$ | $b$ |
| *km.* | | *km.* | | *km.* | |
| 13.9 | −19°.1 | 19.7 | −19°.9 | 28.4 | −22°.8 |

We find, accordingly, in all cases a negative latitude for the maximum $\theta$. It is known, however, that the sun is situated north of the real galactic plane, the distance being about 20 parsecs,[7] or a distance corresponding to a parallax of 0″.05. This indicates a maximum of motion, not in the galactic equator, but in the real galactic plane. The form of the surface for the second group is shown in figure 1.

The surfaces which represent the average radial velocity for the first and second groups are very nearly the same, the most important features being two large terms of second and third order. These are as follows, $b$ and $l$ being the galactic latitude and longitude, respectively:

| | SECOND ORDER | THIRD ORDER |
|---|---|---|
| Group I | $2.59 \cos^2 b \cos 2 (l - 174°.3)$ | $2.90 \cos^3 b \cos 3 (l - 76°.0)$ |
| Group II | $3.18 \cos^2 b \cos 2 (l - 162°.7)$ | $3.10 \cos^3 b \cos 3 (l - 77°.6)$ |

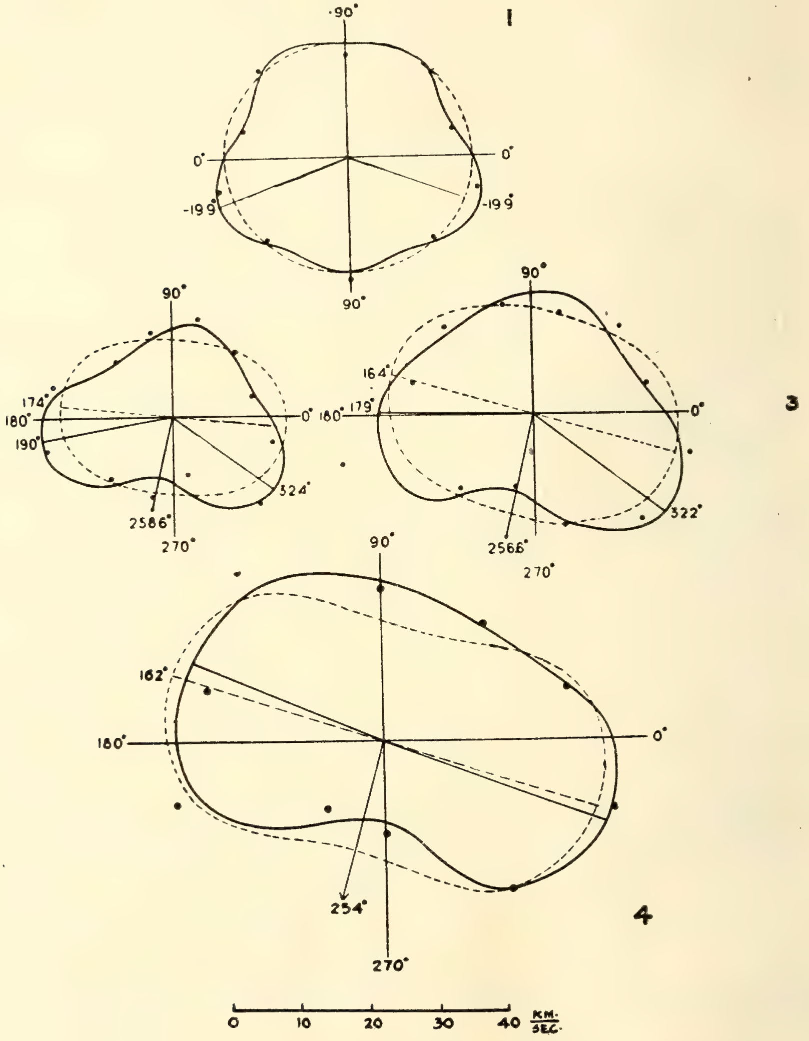
The second order terms define the stream motion, and the existence of the large third order terms shows that the axes of maximum radial velocity do not lie in a straight line.

The two principal maxima of $\theta$ in the two surfaces are

| | $\vartheta$ | $l$ | $b$ | $\alpha$ | $\delta$ | $\vartheta$ | $l$ | $b$ | $\alpha$ | $\delta$ |
|---|---|---|---|---|---|---|---|---|---|---|
| | *km.* | | | | | *km.* | | | | |
| Group I | 19.60 | 190° | + 4° | 109° | −7° | 17.50 | 324° | − 2° | 264° | −33° |
| Group II | 24.23 | 179 | −18 | 84 | −7 | 26.33 | 322 | −19 | 283 | −41 |

We find, therefore, that the axes of largest radial mobility, which may be assumed to correspond approximately to the direction of preferential motion in space, form obtuse angles equal to 134° and 128°, respectively, for the two groups, and that there is a pronounced minimum at longitude 258°. The nearest approach to a symmetrical plane at right angles to the galacticplane has the longitude

258°6 for Group I and 256°6 for Group II.



In figures 2 and 3 are shown the intersections between the radial velocity surfaces and the galactic equator. The dotted curves indicate the intersections when only terms of even order are considered, and the arrows mark the position of the symmetrical planes.

The properties of these surfaces may be explained on the assumption that the motions of the stars depend upon their positions relative to the center of the galactic system. According to Charlier[3] the position of the center as derived from 800 B-type stars is

$l = 236°$   $b = -14°$   Distance = 88 parsecs.

O. R. Walkey[8] finds from 30736 stars of all types the longitude $l = 246°$. These values of the galactic longitude of the center of the stellar system are in fair agreement with the value 258° found for the position of the symmetrical plane. We may, therefore, conclude that the variation of average radial velocity with direction is due probably to a motion of the stars around the center of the galactic system. Moreover, if the stars are moving around the center of the stellar system we should expect a minimum orbital velocity near the center and a maximum at a certain distance from the center.[5] The minimum in radial velocity near longitude 268° is in agreement with this conclusion.

In the case of the stars nearest to us, we should expect the axes of preferential motion to lie nearly in a straight line, since their distance from us is small as compared with their distance from the center.[9] To test this question I have made an analysis of the radial velocities of the stars in Group III, which contains the nearest stars, combining all stars between galactic latitudes $-66°$ and $+66°$. The axes of maximum radial velocity for the resulting curve have the longitudes 157° and 340°, which thus differ by 183°. The longitude of the axis of symmetry is 254°, a value in good agreement with that found for the more distant stars. The intersection of the surface with the galactic plane is shown in figure 4.

The conclusion to be drawn from these results is that stream motion is probably a local effect caused by a preferential motion of the stars in both directions around the center of the stellar system. This might have been expected from the fact that the motions of the two 'drifts' have been found to be in the galactic plane and at right angles to the direction of the center of the galaxy. The deviation of the axes of preferential motion from a straight line furnishes strong evidence in support of this conclusion.

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[1] Adams, W. S., and Strömberg, G., *Mt. Wilson Contr.* No. 131, *Astroph. J., Chicago,* **45,** 1917, (293–305).

[2] Kapteyn, J. C., *Groningen, Pub. Astr. Lab.*, No. 8.

[3] Turner, H. H., *Mon. Not. R. Astr., Soc. London,* **72,** 1912, (387–407).

[4] Eddington, A. S., *Ibid.*, **75,** 1915, (366–376).

[5] Mt. Wilson Solar Observatory, *Annual Report,* 1916, (255).

[6] Eddington, A. S., *Mon. Not. R. Astr. Soc., London,* F. S., 1915, (521–530).

[7] Charlier, C. V. L., *Medd. Lunds Obs., Upsala,* Ser. 2, No. 14, 1916, (31).

[8] Walkey, O. R., *Ibid.*, **74,** 1914, (649–655).

[9] Strömgren, E., *Astr. Nachr., Kiel,* **203,** 1916, (17–24).

# DISEASE RESISTANCE IN CABBAGE

By L. R. Jones

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Communicated by R. A. Harper, December 31, 1917

The cabbage, while ordinarily one of the most vigorous of cultivated plants, is highly susceptible to certain parasitic diseases. The most destructive of these, known as 'yellows,' is due to a soil inhabiting fungus (*Fusarium conglutinans*) which invades the root system. Once introduced this parasite may persist indefinitely in the soil rendering it 'cabbage sick' so that this crop can no longer be grown upon it. One who knows the nature of this parasite and its manner of host invasion would have little hope of its practical control through any of the usual methods of spraying or seed or soil treatment. Extensive experimental trials have justified this conclusion.[1] On the other hand, when we were called upon in 1910 to investigate this disease and its control in Wisconsin, it was these very factors which encouraged the hope that it might be possible to secure disease resistant varieties or strains. The principle involved in such an undertaking is, of course, that of 'the survival of the fittest.' The things essential to any satisfactory progress are that there be among the host plants well marked variations in individual susceptibility or 'resistance' to the parasitic attacks and that this character, whatever its cause, be fixed and transmissible from generation to generation. Another very important matter, from the practical standpoint, is that such resistant characters be combined with those which give the plants economic value. Examination of the worst diseased fields led to the discovery of certain individual plants which developed apparently normally although all neighboring plants were dying or dead from the effects of the parasite. Moreover, among these survivors were to be found representatives of the best commercial types. The first and last conditions were therefore met. Would these characters persist in seed obtained from such plants?

Somewhat over 50 such resistant individuals were selected in the autumn of 1910, seed was grown from them in 1911, and in 1912 this seed planted, each 'head strain' separately, on the 'sickest' available soil. Omitting all details the general results may be summarized by the statement that in every case the selected head strains transmitted in considerable degree their resistant qualities, and certain of them did so in high degree. Thus of the control (non-selected) varieties the majority were killed by the parasite and most of the balance were so diseased that they failed to form heads, whereas the *poorest* of the selected head strains proved decidedly superior to the *best* of these controls, and of the best selected head strain 98% of the plants lived through the season and 93% of them formed heads. The figures which may best be cited are, however, those comparing the head strain which in the behavior of its progeny has since proved to be the best with the unselected control strains.

| | | |
|---|---|---|
| Selected head strain | 96% lived | 80% headed |
| Non-selected controls | 36% lived | 16% headed |

From the best of these heads, selected as the second generation survivors on sick soil in 1912, seed was grown in 1913 and like trials on the same infected soil made again in 1914. In order the better to show the significance of these results the following summary includes the averages of *all* of the head strains selected from the best strain in 1912 together with the results from the *best* head strain. Data are also here included as to the per cent of diseased plants in each strain at the end of the season, the average weight of the heads, and the computed yields in tons per acre.

| SECOND GENERAL TRIALS, 1914 | LIVED | HEADED | DISEASED | WEIGHT PER HEAD | YIELD PER ACRE |
|---|---|---|---|---|---|
| | *per cent* | *per cent* | *per cent* | *pounds* | *tons* |
| Average all selected strains | 99.5 | 94.0 | 5.0 | 4.0 | 12.2 |
| Best selected head strain | 100.0 | 98.0 | 1.5 | 5.5 | 18.8 |
| Average non-selected controls | 46.0 | 24.5 | 81.0 | 2.7 | 2.1 |

A comparison of the 1914 results with each other and with those of 1912 shows clearly certain encouraging things. First, the disease resistant character, whatever may constitute it, is fixed and inheritable; second, there is a distinct improvement in the second generation as compared with the first; third, there is enough variation as between the selected head strains of the second generation to furnish a basis for further possible improvement by continued selection. Perhaps this last idea requires restatement in such a way as to lay the emphasis upon another fact. Since there is a considerable variation within the progeny of even the selected head strains it is necessary to continue the method of growing the seed strains on 'sick' soil for trial and selection in order to maintain the highest standards, and were this to be discontinued there would probably be a tendency toward gradual reversion. Such selections and trials have since been continued in an intensive way by our department with further encouraging results while the more extensive work of seed growing and its distribution (under the name Wisconsin Hollander) has passed to the hands of a committee of practical cabbage growers organized for this purpose.

In this way our own departmental activities, including the investigations of certain graduate students, have been freed for direction to some of the more fundamental questions involved. Among these the following may be defined.

1. Wherein is the difference between the susceptible and the resistant plant? In other words, what constitutes disease resistance?

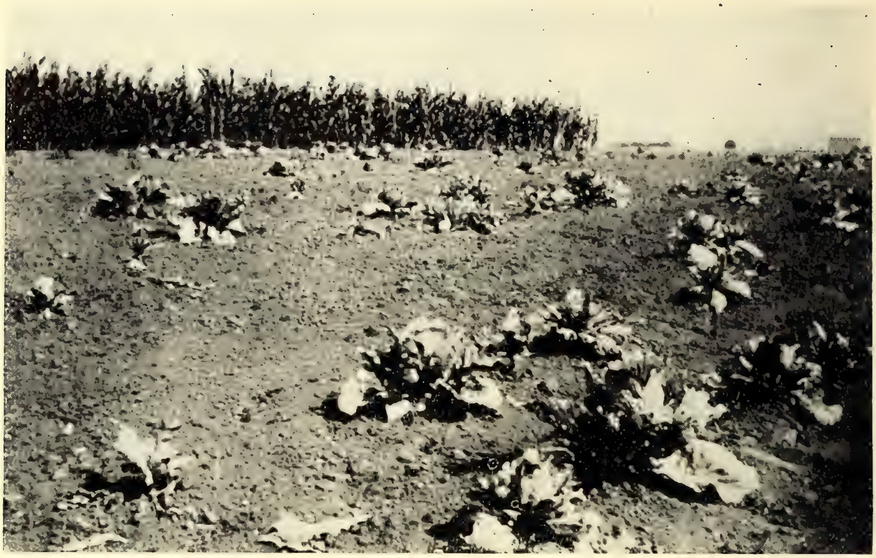

FIG. 1. A FUSARIUM-SICK CABBAGE FIELD

Note that while most of the plants are diseased or dead certain appear normal. Selections of such were made from this field in 1910.

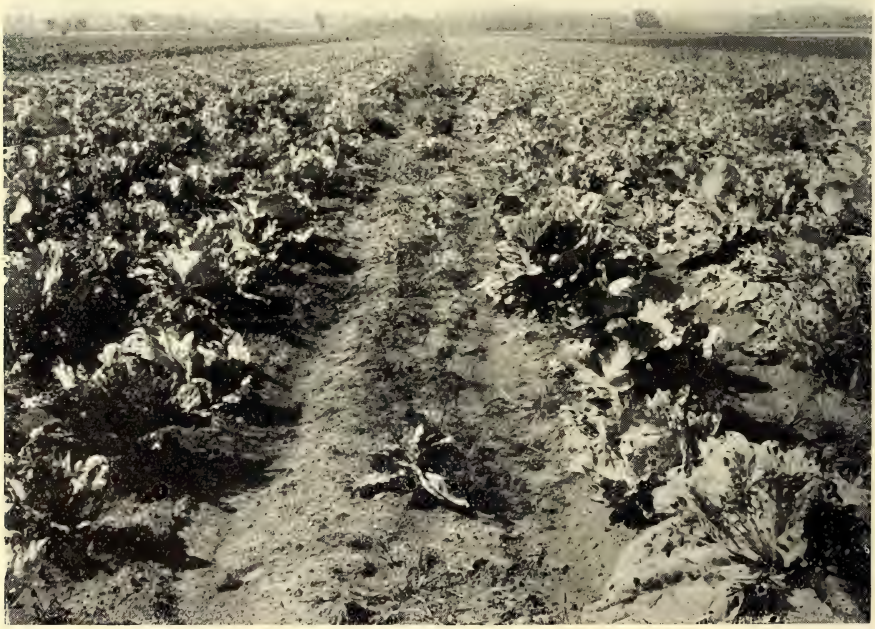

FIG. 2. A TRIAL OF RESISTANT STRAINS ON 'SICK' SOIL IN 1916

The central row, nearly all dead, is a commercial non-resistant strain planted as a control. The remainder of the field is planted with selected resistant strains, the first row at the left being recently selected kraut varieties, Brunswick and All Seasons, the rest being Wisconsin Hollander.

2. How does this factor of disease resistance behave in inheritance? Is it surely transmissible and does it 'Mendelize?'

3. Is the quality of resistance or susceptibility influenced by environmental factors, if so, how? Related to this is the question as to whether such resistance will persist when the strain selected in one locality, e.g., Wisconsin, is grown in a remote locality, e.g., New Jersey.

4. Is the principle general? In other words, is there similar variation in individual susceptibility and resistance to this parasite among other varieties of cabbage than the one considered above? How general is the occurrence of such differences with other host plants and other plant parasites?

While wholly satisfying answers are not yet formulated for any of these questions, such evidence as has been secured may be summarized as follows.

As bearing on the first question, W. H. Tisdale, working in our laboratory, has sought to learn what difference there is in the relations of the parasite to the resistant and the susceptible plants. Since the cabbage is a clumsy plant of slow development he used for his primary investigations the flax which is invaded by a closely similar parasitic Fusarium and which has well differentiated resistant and susceptible strains. Secondary comparative studies with the cabbage, while not yet complete, indicate a general likeness in behavior. He has learned that the difference in resistance is not due to any superficial obstacle since the parasite, which may enter through the root hairs, penetrates these freely in the resistant as well as the susceptible plants. The difference is in the relations of the interior cells of the host and the parasite. In the suceptible plants the parasite penetrates directly to the vessels and then ramifies through them to the destruction of the host. In the resistant individuals, on the other hand, the invasion advances more slowly and before it reaches the vessels is checked and permanently walled off by the development of a corky layer. This reaction may be interpreted in various ways but the one we favor is that the resistant tissues have the ability to restrain the development of the parasite to a greater degree than do the susceptible and so give time for the protective cork formation.

With reference to the second question, Tisdale[2] has also sought to follow the behavior of resistance as an inherited character again using the flax. Biffin, in England, working with wheat, concluded that resistance to the rust parasite (Puccinia) was inherited as a simple Mendelian character, being recessive. With flax the problem proves not so simple. Crossing highly susceptible with resistant strains and testing the progeny shows that resistance is clearly a transmissible character, but the hybrids are in general more or less intermediate in this respect with a tendency for resistance to be dominant. Much variation is found in lines originating from different parents. The indications are that resistance is not a single character but a complex, dependent upon a number of heritable factors.

As to the third question, environmental factors do have a marked influence in determining whether the Fusarium parasite will invade the cabbage. J. C.

Gilman working at Wisconsin and later at the Missouri Botanical Garden found that there is a 'critical soil temperature,' about 17°C., for such invasion. Below this the plants are not parasitized, even in the sickest soils while for some 10° above this the attack becomes progressively more virulent. Correlated with this is the experience that at lower soil temperatures, e.g., in cool summers, there is little disease even with susceptible strains and in hot summers there is a certain amount of infection even with our most resistant strains. It is theoretically possible, therefore, that a strain which was resistant in a relatively cool climate might prove susceptible when transferred to a warmer region. As a matter of field trial, however, these resistant Wisconsin cabbage strains have proved similarly resistant under trials extending from New Jersey to Iowa.

Regarding the fourth question we may speak with considerable confidence also. Our earlier work was confined, as previously suggested, to one cabbage variety, the Hollander. Selections which have since been made from other varieties, both in connection with our own investigations and by others in Ohio, Iowa and Maryland, have given encouraging results. We may be reasonably confident, therefore, that a Fusarium-resistant strain of cabbage may be secured from any vigorous established variety.

If we do not ask for too much in the way of practical application we may safely go farther in the generalization as to the occurrence of disease resistance in plants. Plants are always varying of course, and this includes variations in those factors which make them more or less susceptible or resistant to the attacks of any parasite. Unquestionably constantly increasing recognition of this principle will be given by plant breeders and plant pathologists in seeking to control plant diseases. On the other hand, experience indicates that with parasites of certain kinds including the Fusariums, it is relatively easier in practice to secure disease-resistant strains of host plants than it is with many other parasites, for example the common wheat rust.

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[1] For a more detailed consideration of these matters reference may be made to *Wis. Agric. Exp. Sta. Res. Bul.*, 38, The Control of Cabbage Yellows through Disease Resistance, by L. R. Jones and J. C. Gilman, December, 1915.

[2] Tisdale did his work on inheritance as a graduate student under the direction of Dr. L. J. Cole in the department of experimental breeding, at the University of Wisconsin. His results along these two lines have been formulated for publication in the *Journal of Agricultural Research.*

# IS A MOVING STAR RETARDED BY THE REACTION OF ITS OWN RADIATION?

By Leigh Page

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Communicated by H. A. Bumstead, January 21, 1918

A question of some interest to the astronomer is whether or not a body in motion, such as a star, is retarded by the reaction of its own radiation. For, on the electromagnetic theory of radiation as developed by Maxwell and his followers, a beam of radiant energy is supposed to have a quasi-momentum, such that if a body emits energy in a single direction it will lose momentum and in consequence suffer a reaction tending to push it in the opposite direction. Now if a star is at rest, and in thermal equilibrium, it follows from symmetry that it will radiate equally in all directions, and there will be no resultant impulse. If, however, the star is in motion, classical electrodynamics leads to a greater emission in the forward direction than in the backward, and consequently it would appear at first sight as though there should be a retardation which would ultimately bring the star to rest. The problem has been treated in some detail by Professor Sir Joseph Larmor in the *Proceedings of the Fifth International Congress of Mathematicians*,[1] held at Cambridge in 1912, and he finds the resistance to motion due to the radiation to be

$$F = -vR/c^2$$

where $v$ is the velocity of the star, $R$ the rate of emission of energy, and $c$ the velocity of light.

Apart from its intrinsic interest, Larmor's result is of importance in that it would constitute, if correct, a contradiction between classical electrodynamics and the Principle of Relativity (reference here is to the relativity of constant velocity systems, not to the broader conception of general relativity recently developed by Einstein). It is known, however, that the connection between classical electrodynamics and the Principle of Relativity is very close. Lorentz obtained the relativity transformations in his effort to give the electrodynamic equations for a moving system the same form as for a fixed system, even before Einstein advanced the relativity idea, and the author has shown that the electrodynamic equations can be derived in their entirety and exactly from the kinematical transformations of relativity and the assumption that each and every element of charge is a center of uniformly diverging tubes of strain.[2]

Now to calculate rigorously from the electrodynamic equations the reaction on so complicated a mass as a star would be hopelessly involved. Fortunately, however, the problem can be simplified to the extent of dealing with a single oscillator, i.e., a single vibrating electron, and yet we can obtain a result that will be a perfectly general test of Larmor's expression. For the latter gives the retarding force as a function of the rate of total radiation and the velocity of the radiating body, and of these quantities alone. Hence if the ether exerts a reaction on a group of moving oscillators, it will exert a similar reaction on a single oscillator; and conversely, if there is no reaction on a single vibrating electron due to its drift velocity, there can be none on a group of such vibrators.

A rigorous solution of the problem for this relatively simple case shows the existence of no retarding force. Larmor's result is found to be invalid because of a tacit assumption underlying his reasoning which was introduced substantially in the following manner. In order not to complicate matters by the introduction of terms in the inverse second power, the radiation reaction is calculated by applying the electrodynamic equations to the surface of a moving sphere with the electron as center, whose radius is large compared to that of the electron (though small compared to a millimeter). In this way only those terms in the expressions for the electric and magnetic fields which involve the inverse first power of the distance from the electron need be retained. But the result obtained really gives the force on the electron and the ether inside the moving sphere, not that on the electron alone. Now, if the motion of the electron were undamped, the field inside this moving sphere would remain unchanged, and consequently the force found by Larmor would be that actually exerted on the electron. But as the electron is radiating, its motion must be damped unless energy is supplied from some outside source, and in that case it must be shown that no impulse accompanies the transfer of energy to the electron—a matter of considerable difficulty to treat rigorously. It is far simpler to consider the case of an oscillator left to itself and allowed to radiate at the expense of the energy of its vibration. For this case it is found that the force exerted on the electronic vibrator by the ether inside the moving sphere mentioned above is exactly equal and opposite to that due to the ether outside. Moreover, from the point of exchange of momentum, the law of conservation of momentum demands that

$$\text{Momentum lost by electron} = \text{Momentum gained by ether outside sphere} - \text{Momentum lost by ether inside sphere.}$$

The terms on the right hand side (the second of which is overlooked by Larmor) annul one another. Therefore a single moving oscillator is not retarded by its radiation field, and as already noted we can generalize this result and conclude that a moving body of any size and complexity suffers no retardation as a result of its emission of radiant energy.

In the analytical reasoning leading to this conclusion it is found necessary to develop the complete dynamical equation of the Lorentz electron through terms of the fifth order, for the most general type of motion. Previous derivations of this equation have been confined to some special case, such as quasi-stationary motion in a straight line. The general equation is here published for the first time:

$$F_x = \frac{e^2 f_x}{6\pi a c^2 (1-\beta^2)^{3/2}} - \frac{e^2 \mathbf{f}\cdot\boldsymbol{\beta} f_x}{2\pi c^4 (1-\beta^2)^3} \cdots - \frac{e^2 \dot{f}_x}{6\pi c^3 (1-\beta^2)^2} + \frac{e^2 \ddot{f}_x a}{9\pi c^4 (1-\beta^2)^{5/2}} - \frac{e^2 \dddot{f}_x a^2}{18\pi c^5 (1-\beta^2)^3} \cdots$$

$$F_y = \frac{e^2 f_y}{6\pi a c^2 (1-\beta^2)^{1/2}} - \frac{e^2 \mathbf{f}\cdot\boldsymbol{\beta} f_y}{2\pi c^4 (1-\beta^2)^2} \cdots - \frac{e^2 \dot{f}_y}{6\pi c^3 (1-\beta^2)} + \frac{e^2 \ddot{f}_y a}{9\pi c^4 (1-\beta^2)^{3/2}} - \frac{e^2 \dddot{f}_y a^2}{18\pi c^5 (1-\beta^2)^2} \cdots$$

where the $X$ axis is taken in the direction of motion, the $Y$ axis is in any direction perpendicular to the velocity, $a$ stands for the radius of the electron, $f$ is its acceleration, and $\beta = v/c$ where $v$ is the velocity of the electron and $c$ that of light. The charge $e$ is expressed in Lorentz's unit. It may be remarked that the form of this equation, in so far as it involves $\beta$, is in exact accord with the Principle of Relativity.

The analytical reasoning involved in obtaining these results from the electrodynamic equations will be given in a paper which has been submitted for publication to the *Physical Review.*

---

[1] *Proc. Fifth Int. Cong. Math.*, **1**, 1912, (207).

[2] Relativity and the Ether, *Amer. J. Sci.*, *New Haven*, **38**, 1914, (169).

## *ON ELECTROMAGNETIC INDUCTION AND RELATIVE MOTION. II.*

By S. J. Barnett

Department of Physics, Ohio State University

Communicated by R. A. Millikan, January 19, 1918

1. If a cylindrical condenser is placed in a uniform magnetic field with lines of induction parallel to its axis, and is short-circuited and rotated, together with the short-circuiting wire, about this axis, it becomes charged to a potential difference equal to the motional electromotive force in the wire, or the rate at which the wire cuts magnetic flux. If, however, the condenser and wire are fixed and the agent producing the magnetic field rotates, the relative motion being the same as before, the condenser does not become charged, as was proved[1] by precise experiment in 1912. This is the first case in electromagnetic induction in which the observed effect does not depend entirely on the relativity of the motion.

Without, however, the introduction of hypotheses unproved by experiment it does not seem possible to determine from the experiments on rotation what would happen in the case of translatory motion, or to obtain from them any answer to the question of the existence of the aether. The present investigation was undertaken with the hope of shedding some light upon these problems.

2. If an air condenser with horizontal parallel plates, short-circuited and placed in a uniform magnetic field whose lines of induction are parallel to the plates, is moved, together with the short-circuiting wire, in a direction parallel to the plates and perpendicular to the lines of induction, it will become charged to a potential difference equal to the motional electromotive force, which will be denoted by $E$, in the wire, the charges (provided, as will be assumed, that edge effects are negligible) being restricted to the inner or opposing faces of the plates.

3. If the region below the plane of the upper surface of the lower condenser plate were filled with a medium of zero permeability, the tubes of magnetic induction would be confined to the region above this plane, and the condenser would become charged exactly as before for the same motion.

4. If, with the arrangement of §3, the condenser and wire were to remain fixed and the agent producing the magnetic field were to move, the relative motion being the same as before, the results to be expected would be different according to the hypothesis adopted with respect to the aether:

A. If there is no aether, and the principle of relativity is valid, the condenser would become charged exactly as in §2 and 3.

B. If the aether exists, the electric intensity produced by the motion of the tubes of induction in the aether between the condenser plates would be equal and opposite to the field intensity there due to the potential differences produced by the electromotive force in the short-circuiting wire, and *there would thus be no charge on the lower condenser plate.*

Thus we should have a method of discriminating between the two hypotheses.

5. There being of course no medium of zero permeability in existence, I have tried to secure an arrangement equivalent to that of §4, so far as the effect under investigation is concerned, by using an artifice analogous to the electric guard-ring.

Two similar electromagnets, referred to as the upper and lower magnets, with their coils in series are used to produce the field. The four poles are all alike, each being a rectangular parallelepiped with the largest sides horizontal and parallel to the condenser plates. The lower magnet and condenser are fixed to the floor. The upper magnet forms the bob of a huge pendulum swung from the ceiling, and has translatory motion parallel to the condenser plates when in its lowest position. When the upper magnet is in this position, the upper and lower poles are about a centimeter apart, and the upper surface of the lower condenser plate is symmetrically located with reference to all the poles and the other parts of the magnets. The (now) uniform field containing the condenser is conceived to be divided into two parts as follows: an approximately fixed field beneath the plane of the upper surface of the lower plate, and above this plane a field whose tubes of induction move approximately with the speed of the upper magnet. The induced electromotive forces are thus restricted to the same regions as in the imaginary experiments of §4.

6. In the experimental arrangement the lower condenser plate is (ordinarily) connected by a wire with one quadrant pair of an electrometer. The other pair and the upper plate are connected (the latter through a key at will) to a thin metallic case forming a practically complete electric screen about the condenser-electrometer system. The wire joining the condenser and electrometer can be connected to the case by closing an electrically operated key *K*, thus short-circuiting both condenser and electrometer. A suitable calibrating arrangement is provided.

7. The calibration experiments give the deflection $D$ which would be produced by charging the condenser to the potential difference $E$ of §2 (the upper plate being disconnected from the case for the purpose), insulating the lower plate and electrometer by opening $K$, and reconnecting the upper plate to the case.

8. The principal experiment, together with experiments for determining extraneous effects, gives the deflection $d$ due to any charge on the lower plate produced when the upper magnet moves (with known speed) past the center of the fixed system, opening the key $K$ at the center of the motion. An extended series of experiments shows that $d/D$ is zero within the limits of the experimental error.

The investigation thus appears to support the hypothesis of §4, B, which assumes the existence of the aether, and to be inconsistent with the principle of relativity.

I am indebted to Mrs. Barnett for a great deal of help in making the experiments, to Mr. Freund for mechanician's assistance, and to the Carnegie Institution and Professor Pegram for some of the instruments used.

A detailed account of the work will be submitted for publication to the *Physical Review*.

---

[1] *Barnett, S. J., Physic. Rev., Ithaca, N. Y.*, **35**, 1912, (323–336)

## *NATIONAL RESEARCH COUNCIL*

### REPORT OF THE COMMITTEE ON ANTHROPOLOGY

The Committee on Anthropology was one of the first constituted under the National Research Council. Its personnel is limited to representatives of Physical and Medical Anthropology which branches it was recognized could be of direct assistance in national preparedness. Its membership comprises William H. Holmes, Chairman; Ales Hrdlicka, Secretary; Dr. Charles B. Davenport, Carnegie Institution; Dr. Frederick L. Hoffman, Chief Statistician, The Prudential Insurance Company; Dr. E. A. Hooton, Anthropologist of Harvard University; Dr. George M. Kober, Dean of the Medical Department, Georgetown University; Mr. Madison Grant, Trustee, American Museum of Natural History; and Dr. Tom A. Williams, Psychiatrist and Anthropologist.

*Activities of the Committee.*—On February 16, 1917, certain recommendations were formulated, which under the heading "A National Anthropometric Survey," were submitted to the Executive Committee of the National Research Council by Dr. Charles D. Walcott, first vice president of the Council.

In April additional suggestions were laid before Dr. V. C. Vaughan, representing the Council. These suggestions related to changes in the physical requirements of the Army, the advisability of proper regulation of the instruments and methods to be used in the measurement of recruits, the need of additional anthropological observations on drafted men in the camps, and the preservation of valuable data and specimens on post-mortem material. Later these recommendations were formally submitted to the National Research Council.

In May these suggestions were submitted in final form. In June they were presented before the Medical Branch of the National Defense Council, and in August were published in brief form in the *Proceedings of the National Academy.* In June the Committee made further definite proposals for the standardization of the instruments used in the examination of recruits and for the regulation of methods.

Finally during August and September a proposal was made for sending a trained anatomist to the base hospitals in Europe for the purpose of collecting much needed observations as well as specimens for further study. At the same time the Committee called attention to the probability that our men in France would discover in their digging operations valuable archeological and skeletal material, which should be preserved to science; and further, the Committee offered its services in dealing with the many problems relating to race, nationality and language which are bound to be encountered by the Commission that will have charge for the United States of peace negotiations.

The results of the activities of the Committee unfortunately are not wholly encouraging. It finds on the one hand a number of simple, practical and from every standpoint desirable steps to be taken, and on the other the great difficulty of securing favorable action by the proper authorities. The only recommendation of the Committee so far adopted is that relating to the necessity of reducing the minimum physical requirements in recruiting. Practically nothing has been done towards the improvement of the methods and instruments employed in the examination of drafted men. The blanks used for the examination of recruits are essentially the same as of old. The *Manual of Instructions* for examiners was printed but no directions were included respecting the methods or instruments to be used in the measuring. The Committee has found that a large percentage of the instruments used for the examinations are more or less defective; that the methods of taking the measurements are regulated by individual opinion, necessarily resulting in many errors; and that in cases the measuring is relegated by the examining physician to enlisted men, without adequate supervision. It is painfully evident that no improvement has been effected in this important matter since the Civil War, and that the millions of measurements to be taken will be entirely unreliable for scientific purposes. Thus we lose demographic data of the greatest value; data which would have given to science and to government for the first time reliable information regarding the physical characteristics of the American people in different parts of the country, in different occupations, and under different environments.

During the latter part of August and the first part of September the Secretary of the Committee took up with Major Vaughan and Major Millikan, the Acting Head of the National Research Council, the question of sending a trained anatomist or anthropologist to Europe, to be attached to the American base hospitals and to take charge of the collecting of data and objective material relating to post-mortem cases. As many races will be represented in the United States Army, this step is of particular importance. Both normal and pathological specimens should in suitable instances be preserved, to be deposited for future studies in the National and the Army Medical Museums and possibly other institutions. It was recommended that the specialist sent should be a man of high training and experience and that he be commissioned in the Medical Service of the Army so that his work could be carried on in a regular manner and not dependent on favors. It was suggested by the Committee that if necessary the salary and working expenses of this officer might be provided for, but this suggestion met with a stumbling block in the disinclination of those who are concerned, to commission the specialist who might be recommended.

The next step suggested by the Committee relates to the preservation of any finds of ancient human material, archeological or skeletal, that may be discovered by the American Army in their trench digging and excavations in France. France, as is well known, was the home par excellence of man from the haziest antiquity. Most of our knowledge relating to his evolution has come from collections made in France, and much more material doubtless still lies in French soil. During the extensive digging of trenches and other excavations the men of our Army are more than likely to come across such precious remains and steps should be taken for their preservation. The armies on the western front have already made such discoveries.

Notwithstanding the discouragement so far encountered by the Committee a great deal in the line of its recommendations can still be accomplished. Many more drafted men will be examined and the opportunities for observations in the camps are multiplying. The collection of post-mortem data and material can be initiated at any time before the actual fighting commences. Our great problem now is how shall we proceed in order to be more successful in these directions in the future than we have been in the past, and how can our recommendations be made effective?

W. H. HOLMES, *Chairman.*
ALES HRDLICKA, *Secretary.*

## *NOTICE OF BIOGRAPHICAL MEMOIRS*

The following biographical memoir has been published by the Academy since the last notices of such memoirs appeared in the August 1917 number of the Proceedings.

JOHN SHAW BILLINGS (1838–1913). By S. WEIR MITCHELL, With The Scientific Work of John Shaw Billings. By FIELDING H. GARRISON. *Biographical Memoirs of the National Academy*, **8**, pp. 375–416.

The first biographer sketches in generality, and with the sympathy of a personal friend, the life work of John Shaw Billings in its early struggles, in the army during the Civil War, in the Army Medical Museum, in the National Academy, and in the Astor-Tilden-Lenox Library of New York. Dr. Garrison, though not neglecting personal characteristics, treats primarily the achievements of Dr. Billings in the six fields of his activity, military and public hygiene, hospital construction and sanitary engineering, vital and medical statistics, medical bibliography and history, advancement of medical education and the condition of medicine in the United States, and in civil administration; he also gives an account of the scientific contributions originating with Dr. Billings either directly or through his pupils, but attempts no formal bibliographical list of the published writings.

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PROCEEDINGS
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Volume 4 MARCH 15, 1918 Number 3

# THE EFFECT OF ARTIFICIAL SELECTION ON BRISTLE NUMBER IN DROSOPHILA AMPELOPHILA AND ITS INTERPRETATION

By Fernandus Payne

Zoological Laboratory, Indiana University

Communicated by H. S. Jennings, January 7, 1918

About two years ago I began an experiment to test the effect of artificial selection in a bisexual form. Drosophila ampelophila was chosen after a careful search through the list of material which could be bred in the laboratory. There were two principal reasons for using this material. First, it is easily bred, and second, because of the four sets of linked genes described by Morgan and others, it is possible to better interpret how the results of selection have been accomplished. I wish to emphasize that the interpretation of the results is the important part of the problem. Practically every one admits that selection may be, in certain cases, effective.

The present work is not a repetition of McDowell's, although the character, bristle number, has been used. He used the bristles on the thorax, while I have used the bristles on the scutellum. Some of our final conclusions may agree, yet, the work is very different and I believe my own conclusions have been carried to a more definite termination. McDowell did not attempt to link up the factors which he believed the cause of extra bristle number with any other genes.

The experiment was started by mating a female with one extra bristle on the scutellum to a normal male (four bristles is the normal number). Both flies were taken from a mass culture which had been kept in the laboratory for about three months. Counts in this mass culture before the experiment began gave 612 normal flies and the one female with one extra bristle. The result of this cross (female five by normal male) was 226 normal flies and two females with one extra bristle. These two $F_1$ females with one extra bristle were mated to $F_1$ normal brothers. These two pairs gave $F_2$ offspring as follows: 935 normal, 39 with one extra bristle, and four with two extra bristles, a ratio of extras to normals of 1: 21.7. From these $F_2$ offspring, the flies with extra bristles were mated. This method of selecting the high grade parent has been continued for 38 generations. A rise in the per cent of extra bristled flies and also in the mean bristle number has been produced until in the last generations of selection no normal flies were found and the mean number of bristles in the twenty-ninth generation was 9.084. From the twenty-ninth to the thirty-eighth generation the mean remained practically the same. The highest bristle number found in any one individual fly was 15.

I cannot give details, but I think these figures are sufficient to show that selection has been effective. How then has it been effective? Have the results been produced by selecting merely somatic variations? Have they been produced by selecting the variations of a single gene, or have they been produced by getting rid of or by piling factors? The possibility that selection can act upon somatic variations, I believe, can now be dismissed without much consideration. Practically every one admits that variations must be germinal in order to be inherited. As to the second and third possibilities, my evidence is in favor of the third.

In the selection line the bristle number in the females is slightly higher than in the males. This indicated that there might be a sex-linked factor present, which when homozygous produced a different effect than when heterozygous. An experiment was devised to eliminate the X-chromosome but retain the others. This was done as follows: A male from the high selection line was mated to a bar female (bar is a sex-linked dominant). The $F_1$ males will get their X-chromosome from the bar female and hence will be bar. Of the other three pairs of chromosomes, one of each pair will come from the high selection line and the other from the bar line. Some of these $F_1$ males have extra bristles. These were mated to bar females from stock. All flies from this cross will get their X-chromosomes from the bar line. Some of them may get one member of each of the second, third, and fourth pairs from the high selection line. Since extra bristle number is partially dominant to the normal, such flies might have extra bristles if there are any genes for extra bristles in the second, third, or fourth chromosomes. Some of the flies from this cross did have extra bristles. They were mated and the line has been inbred to see whether the bristle number could be increased until it reached the mean in the high selection line. If so, then the X-chromosome could carry no gene for extra bristles. If the mean could not be raised, then it would indicate that the X-chromosome carried such a gene. Five generations of inbreeding has failed to raise the mean above 5.2.

Further, by crossing the high selection line to eosin, miniature and to eosin ruby forked (sex-linked characters), and mating the $F_1$ females back to the recessives, it comes out clearly that a gene influencing bristle number is located in the X-chromosome, somewhere near eosin. Of the cross-overs between eosin and miniature and between eosin ruby and forked, miniature and forked flies have a higher bristle number than the eosin, and eosin ruby.

The high selection line was also mated to black, pink, bent stock. The gene for black is in the second, that for pink in the third, and that for bent in the fourth chromosome. The $F_1$ flies were mated inter se and the $F_1$ males were mated back to black, pink, bent females. The normal, the black, and the bent flies from these crosses had a much higher bristle number than the pink flies. This fact indicated that there probably was a factor for bristle number in the third chromosome. To test this possibility the high selection line was mated to sepia spineless kidney sooty rough stock. The genes for these characters are located in the third chromosome. The $F_1$ females were mated to sepia spineless kidney sooty rough males. If crossing-over had occurred, this cross should have enabled me to link up the gene for extra bristles (if one is present) with one of the other genes. Unfortunately, only a low percentage of crossing-over occurs and hence the cross yielded no results.

Another method of analysis was used. A female from the high selection line was crossed to a sepia spineless kidney sooty rough male. The $F_1$ male from this cross was mated to a sepia spineless kidney sooty rough female. The offspring are normal females and males, and sepia spineless kidney sooty rough females and males. Some of the normal females and males and some of the sepia spineless kidney sooty rough females had extra bristles. None of the sepia spineless kidney sooty rough males had extra bristles. When the chromosomes of these flies are analyzed, it is found that the normal females may get one member of each of the second, third, and fourth chromosome pairs from the selection line and must get one of the X-chromosomes from this line. The normal males may get one chromosome of each of the second, third, and fourth chromosome pairs from the selection line, but they get their X-chromosome from the sepia, spineless, kidney, sooty, rough line. The sepia spineless kidney sooty rough females get both their third chromosomes from the sepia, spineless, kidney sooty rough line and may get one member of each of the second and fourth pairs from the selection line. They get one X-chromosome from the selection line and one from the other. The sepia spineless kidney sooty rough males, however, get their X-chromosome and both third chromosomes from the sepia spineless kidney sooty rough line and may get one chromosome of the second and fourth pairs from the selection line. Since these sepia spineless kidney sooty rough males have no extra bristles, it would seem that the X- and the third chromosomes carry the factors for extra bristle number.

A back selection line started from the eleventh generation and carried for twenty-five generations, changed slightly in the first few generations of selection and then remained practically unchanged. A mass culture started a year ago has not returned to the normal.

My conclusion then is that there are, at least, two factors for extra bristle number and that one of these is located in the first and one in the third chromosome. I am inclined to think there are more than two. It seems probable that one of these factors was present at the beginning of selection and that the others occurred as mutations during the course of the experiment.

The complete data and analysis will be given in the final paper.

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# *THE REACTIONS OF THE MELANOPHORES OF AMIURUS TO LIGHT AND TO ADRENALIN*

By A. W. L. Bray

Zoölogical Laboratory, Museum of Comparative Zoölogy, Harvard College*

Communicated by G. H. Parker, January 8, 1918

The following is a brief summary of results obtained in experiments to test the reactions of the melanophores in the skin of Amiurus to light and to adrenalin. The fishes tested were kept under as normal conditions as possible and no fish was used until it had become accustomed to its laboratory surroundings and to handling.

The melanophores of Amiurus contract under the influence of light of ordinary intensity and expand in the dark. Fish kept in white porcelain aquaria contract the melanophores to a much greater extent than do animals kept in black lined aquaria under similar conditions of overhead illumination. When the eyes are removed from normal healthy animals the latter remain dark under all light conditions. There are considerable differences in individuals with respect to the power of contraction of the melanophores. Some fishes become extremely light owing to complete contraction of the pigment cells; others, under similar conditions, never become so light. This cannot be explained entirely by the relative numbers of melanophores per unit area of body surface, as was shown by several counts. It may be due, possibly, to differences in nervous excitability.

If some 'sensitive' individuals are raised to a high pitch of nervous irritability, the melanophores are contracted to the maximum extent, and remain in this condition for a protracted period, even when the fishes are placed in the dark. One such fish was kept in the dark for three weeks and no expansion was observed in the melanophores. These cells appeared to be in a condition comparable to tetanus.

When such an 'excited' animal, with melanophores contracted, is etherized, the pigment cells expand and the fish becomes dark. On coming out of the ether, the melanophores contract slightly, but if the fish is now placed in the dark, the pigment cells gradually assume the expanded condition. If, however, the eyes are removed whilst the fish is under ether, there is no return to the contracted condition when the animal is subsequently kept in the light. The fact that there was no return to the contracted condition of the melanophores after the eyes were removed, both in normal and 'excited' animals, suggests that there is an adaptive significance to be attached to the reactions of the melanophores, and that these reactions are mediated by the nervous system through the eyes.

Excised portions of the skin of Amiurus with melanophores expanded, were immersed in solutions of adrenalin of various strengths, others in tap water and still others in physiological salt solution, and the rates of contraction of the melanophores observed under the microscope.

| Strength of adrenalin solution | Average time for maximum contraction |
|---|---|
| 1 : 5000 | Almost immediately |
| 1 : 10,000 | 4 to 6 minutes |
| 1 : 100,000 | 6 to 8 minutes |
| 1 : 1,000,000 | 16 to 40 minutes |
| 1 : 2,000,000 | 120 minutes |
| 1 : 5,000,000 | Partial contraction in 3 hours |

If an excised portion of the skin with melanophores contracted is placed in ether water, the melanophores expand slowly, whilst the controls, in physiological salt solution or in tap water, show no change.

Intra-muscular injections of adrenalin were made, and others of physiological salt solution as controls. The results on the melanophores were in every way comparable to those found for the excised portions of the skin; that is, the melanophores contracted after injection of adrenalin at rates varying with the strength of the injection. The minimum dilution found effective was 1: 5,000,000. An injection of 1: 10,000,000 caused local contraction of the melanophores in the area around the point where the injection was made, but not in other parts of the skin.

The effect of adrenalin disappears after a time varying with the strength of the injection and ranging from one to several hours.

If a fish, which has been injected with 1: 1,000,000 part of adrenalin, is etherized when it has reached a condition of maximum contraction of the melanophores, there is a perceptible darkening, due to partial expansion of the melanophores. On coming out of the ether the fish returns to the light condition, that is, its melanophores contract. If the fish is subsequently kept in the dark the melanophores gradually expand.

Some fish were removed for a time from the water and two pieces of filter paper, one moistened with physiological salt solution and one with a 1: 100,000 solution of adrenalin were placed on the skin. After fifteen minutes, both were removed. The area of the skin covered by the filter paper moistened with salt solution had become lighter, but darkened rapidly on removal of the paper. The portion covered by the paper moistened with adrenalin solution was quite light, and remained so for about three hours. The influence of the adrenalin spread only slightly to adjacent parts, but a light band appeared in some cases where a little of the solution had run down from the paper.

From the above results it is seen that the melanophores in the skin of Amiurus react to direct stimulation by adrenalin. They are also subject to nervous control, and this control is mediated through the eye. There is also a suggestion of the secretion of a hormone under certain conditions and of its influence on the melanophores.

* Contributions from the Zoölogical Laboratory of the Museum of Comparative Zoölogy at Harvard College, No. 306.

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# *FURTHER EXPERIMENTS ON THE SEX OF PARTHENOGENETIC FROGS*

By Jacques Loeb

Rockefeller Institute for Medical Research

Communicated January 23, 1918

It seemed necessary to furnish proof that by the methods of artificial parthenogenesis not only normal larvae can be produced from unfertilized eggs but that these larvae can also develop into full sized normal adults. This task is difficult to accomplish in sea urchins and thus far only Delage has reported that he has succeeded in raising one parthenogenetic larva of a sea urchin to the sexually mature form.

The possibility of producing artificial parthenogenesis in the eggs of the frog by the method of puncture, as demonstrated in the experiments of Guyer and of Bataillon, seemed more promising. The writer has made use of this method for deciding the question whether such frogs can reach the adult size, and determining their sex. He has now raised twenty leopard frogs to an age of from ten to eighteen months, and nine of these frogs are still alive. Some of these male frogs have reached the full size of the adult male leopard frog. *We are, therefore, entitled to say that the frogs produced by artificial parthenogenesis can develop into adults of full size and of an entirely normal character.*

Loeb and Bancroft[1] tried to ascertain the sex of a parthenogenetic frog immediately after metamorphosis but found the gonads in the intermediate stage, i.e., testes containing a few eggs, though it was obvious that the frog was developing into a male. It was clear that older frogs were needed for the decision of the problem of sex. The writer has been able to ascertain the sex in nine frogs of the age of from ten to eighteen months, and in all of these the ambiguity inherent in the younger frogs had disappeared. He has already reported that the first two of these parthenogenetic frogs has normal mature testes containing fully developed spermatozoa.[2] No eggs were found in these testes.

The next four frogs examined were also males, so that the problem seemed settled when a year ago last summer one parthenogenetic frog, sixteen months old, was found whose gonads were macroscopically and microscopically well developed ovaries. The next frog was again a male. Although the possibility of an error in method seemed excluded the writer did not wish to publish the fact that both sexes appear in parthenogenetic frogs without having checked the result by a new series of experiments.

These experiments were started in February, 1917. The same precautions as in the older experiments were used. Copulating females which had not yet laid any eggs were separated from their males and kept separated for at least twenty-four hours. The females were repeatedly washed with water during the time of isolation, and directly before the experiment were submerged in 90% alcohol and left there to die. They were taken out, their abdominal cavity was opened with sterilized instruments and the oviduct laid bare. The eggs were taken out from the oviduct with sterilized instruments, and precautions were taken that the eggs did not come in contact with the hands of the experimenter or with the skin or outside of the frog. Alternate lots of about 50 to 100 eggs were punctured or kept untreated as controls. None of these non-treated eggs ever developed. From the punctured, unfertilized eggs ten developed into frogs, of which nine are still alive. The tenth was killed December 21 and the microscopic examination of its gonads showed that it was a female. This leaves then no doubt that both sexes can be produced from the unfertilized eggs of the frog. We have thus far obtained seven male frogs and two females, while the determination in two was missed by accident.

How can we account for the production of both sexes? The diploid number of chromosomes in the frog seems to be 26, according to Swingle,[3] and, therefore, the haploid number 13. The question then arises: Do we find the diploid or haploid number of chromosomes in the cells of the parthenogenetic frog? Brachet[4] found the diploid number in the somatic cells of a parthenogenetic tadpole eighteen days old, but, of course, it was out of the question to ascertain the sex of the tadpole.

The gap can be filled by counting the chromosomes in the fully developed parthenogenetic frogs. Thus far the sections of the testes of only one of the writer's parthenogenetic frogs have been examined cytologically. This male was seventeen months old, had reached the full size of the adult, and had large testes with ripe spermatozoa. Prof. R. Goldschmidt, who was good enough to examine some of the sections, counted over 20 chromosomes, and there can be no doubt that this parthenogenetic male frog possessed the diploid number of chromosomes. The writer has not yet been able to ascertain whether the nuclei of the female frogs have the haploid or diploid number.

It is not known whether the female or male is homozygous for sex in the frog. If the female were homozygous it would mean that the haploid number of chromosomes would be $12 + x$ and the diploid $24 + 2x$. In this case only a female could have the diploid number since $2x$ would determine a female. Since we find the diploid number of the male parthenogenetic frog the assumption of homozygosity of the female is inadequate if not excluded. If we assume that the female is heterozygous for sex, and that it has the chromosome constitution $24 + x + y$ (where $y$ may be missing), the male must have the chromosome constitution $24 + 2x$. The haploid number in the egg would be[5] $12 + x$, and the diploid number either $24 + 2x$ or $24 + x + y$. The diploid number $24 + 2x$ would give rise to a male, while a female might be produced by either the haploid number $12 + x$ or the diploid number $24 + x + y$. It is, therefore, of some interest to find out whether or not the female has the haploid number $12 + x$ chromosomes. It is useless to enter into further speculation until this point is decided, which the writer hopes may be possible in the near future.

*Summary.*—The author has raised twenty leopard frogs produced by the methods of artificial parthenogenesis from unfertilized eggs to the age of from ten to eighteen months. Nine of these frogs are still alive. Some have reached the size of the full grown normal adult male. Both sexes are represented among the parthenogenetic frogs. Seven of the nine older frogs whose gonads were examined were males, and two were females. The parthenogenetic males possess the diploid number of chromosomes.

[1] Loeb, J., and Bancroft, F. W., *J. Exp. Zoöl., Wistar Inst., Philadelphia,* **14,** 1913, (275); **15,** 1913, (379).

[2] Loeb, J., these PROCEEDINGS, **2,** 1916, (313); *The Organism as a Whole,* New York, 1916.

[3] Swingle, W. W., *Biol. Bull., Wood's Hole,* **33,** 1917, (70).

[4] Brachet, A., *Arch. Biol., Paris-Bruxelles,* **26,** 1911, (362).

[5] The other haploid number $12 + y$ may be left out of consideration for the present since it is possible that such eggs may not be able to develop.

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# *THE RESOLVING POWERS OF X-RAY SPECTROMETERS AND THE TUNGSTEN X-RAY SPECTRUM*

BY ELMER DERSHEM

DEPARTMENT OF PHYSICS, UNIVERSITY OF ILLINOIS

Communicated by R. A. Millikan, February 11, 1918

This work was undertaken at the University of Iowa with the purpose of determining the wave lengths and the number of lines in the X-ray spectrum of tungsten with greater precision than had heretofore been done.

The method adopted was the well known photographic one in which the crystal is slowly rotated so that it will progressively pass through all the angular positions which are required for reflection of the incident X-rays as demanded by the formula $n\lambda = 2\,d \sin \theta$, in which $n$ is the order of the spectrum, $\lambda$ the wave length, $d$ the grating constant, and $\theta$ the glancing angle of reflection.

Some of the conditions affecting the resolving power of an X-ray spectrometer, that is the ability of the instrument to separate two waves of nearly the same length may be derived by the aid of the diagram (fig. 1).

Assume a source, i.e., a slit, of width $s$ and a crystal of thickness $t$ and assume that the absorption in the crystal is not so great but that some of the rays may penetrate entirely through the crystal and being reflected from the planes on the lower side again traverse the crystal and finally reach the photographic plate $B'A'C'D'$. It is easily seen that there will be an image on the plate equal in width to the width of the source $s$, due to reflection from the upper surface alone. In addition there is a widening of the line due to the part reflected from the lower planes equal to the line $DE$ which is drawn from $P$ perpendicular to $AA'$. Then since $DF = t =$ the thickness of the crystal, $t = AD \sin \theta$ and $AD = DE/\sin 2\theta$ and by substitution $t = DE/\sin \theta = \frac{1}{2} DE/\cos \theta$. Whence $DE = 2t \cos \theta$.

Since $DE$ is the width of beam due to penetration into the crystal the total width of beam is $s + 2t \cos \theta$ and this is the width of line on the photographic plate.

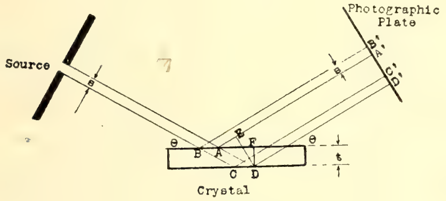


FIG. 1

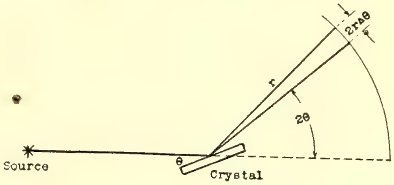


FIG. 2

In order to resolve two lines of nearly the same wave length it is necessary that their images on the photographic plate should not overlap, that is the centers of their images must be further apart than the width of beam, $s + 2t \cos \theta$.

Assume two wave lengths, $\lambda$ and $\lambda + \Delta\lambda$, then to find how small $\Delta\lambda$ may be and still have the two wave lengths clearly separated on the plate: Using the formula $n\lambda = 2d \sin \theta$ let $\lambda$ take on a small increment $\Delta\lambda$ and $\theta$ the corresponding increment $\Delta\theta$. Then by differentiation we obtain $n\Delta\lambda = d \cos \theta \Delta\theta$.

From figure 2 we see that the angle of the crystal must be changed by the amount $\Delta\theta$ in order to reflect the wave of length $\lambda + \Delta\lambda$ instead of the one of length $\lambda$ and that the reflected ray being rotated through twice this amount is rotated through the angle $2\Delta\theta$. If the distance from the crystal to the plate is $r$ then the distance the beam has moved along the plate in changing from $\lambda$ to $\lambda + \Delta\lambda$ is $2r\Delta\theta$ and this distance must be greater than the width of beam, $s + 2t \cos \theta$. Thus

$$2r\Delta\theta > s + 2t \cos \theta$$

But

$$\Delta\theta = \frac{n\Delta\lambda}{2d \cos \theta}$$

Whence

$$\frac{nr\Delta\lambda}{d \cos \theta} > s + 2t \cos \theta,$$

$$\Delta\lambda > \frac{d \cos \theta}{nr} (s + 2t \cos \theta).$$

Defining, as usual, the resolving power to be $\lambda/\Delta\lambda$, we have by dividing $\lambda$ by each side of the inequality

$$\frac{\lambda}{\Delta\lambda} < \frac{nr\lambda}{d \cos \theta\ (s + 2t \cos \theta)}$$

From this it is apparent that the resolving power may be increased by increasing the order of the spectrum and the distance between the crystal and the plate and also by decreasing the width of the source and the thickness of the crystal. To increase the resolving power by any of these means results in a loss of intensity which must be compensated for by an increased time of exposure. To secure the best results in any given case requires a selection by experience of the best relative values of these quantities which will depend upon the kind of crystal used and the hardness of the X-rays.

It is also apparent by an inspection of figure 1 that the true position of the line on the photographic plate is to be obtained by measuring to the outer or most deviated side of the image and then subtracting one half of the width of the source. This does not in general coincide with the position of the most intense part of the image and since the point of greatest intensity is the one obtained by an ionization chamber method the latter can never give results of the greatest accuracy.

In the experimental work the endeavor was made to obtain as high resolving power and as accurate measurements as possible. A Coolidge tube with a tungsten target was used with a rock salt crystal to obtain the results given in the table. These results are certainly accurate to within 0.1% in the case of the $L$ radiations and 0.8% in the case of the $K$ radiations.

By the use of a crystal of rock salt which was first waxed to glass and then ground to a thickness of 0.019 cm. the widening of the $K$ lines due to penetration into the crystal was reduced to such an extent as to cause the doublets to be clearly separated in the spectrum of the first order and this is not possible if the thickness of the crystal is not limited.

In the case of the $L$ group of lines the resolving power as defined by the above formula was less than 170 but nevertheless 19 separate and distinct lines were obtained and this very naturally suggests that if it were possible to obtain such resolving powers in X-ray spectroscopy as have been obtained in

*The Tungsten X-ray spectrum*

| GLANCING ANGLE OF REFLECTION FROM ROCK SALT | WAVE LENGTH IN ÅNGSTROM UNITS | REMARKS |
|---|---|---|
| Lines of the L group | | |
| 15° 16.3′ | $1.482^8$ | Weak |
| 15° 9.9′ | $1.472^2$ | Strong |
| 14° 34.5′ | $1.416^3$ | Very faint |
| 13° 19.9′ | $1.297^7$ | Medium |
| 13° 13.1′ | $1.286^8$ | Very faint |
| 13° 7.7′ | $1.278^4$ | Strong |
| 12° 54.4′ | $1.258^6$ | Medium |
| 12° 44.6′ | $1.241^6$ | Strong |
| 12° 31.3′ | $1.220^2$ | Very faint |
| 12° 24.8′ | $1.209^8$ | |
| 12° 4.5′ | $1.177^3$ | |
| 11° 34.4′ | $1.129^2$ | Weak |
| 11° 13.3′ | $1.095^3$ | Strong |
| 10° 57.9′ | $1.070^5$ | Faint |
| 10° 54.3′ | $1.064^8$ | Medium |
| 10° 50.5′ | $1.058^7$ | Medium |
| 10° 40.6′ | $1.042^7$ | Very faint |
| 10° 29.8′ | $1.025^3$ | Medium |
| 9° 22.0′ | $0.915^9$ | Bromine absorption line |
| 7° 12.9′ | $0.706^8$ | Medium |
| 4° 55.5′ | $0.483^3$ | Silver absorption line |
| Lines of the K group | | |
| 2° 9.7′ | $0.212^4$ | Strong |
| 2° 6.8′ | $0.207^6$ | Strong |
| 1° 52.0′ | $0.183^4$ | Strong |
| 1° 49.0′ | $0.178^4$ | Medium |

the case of light by the aid of the grating and echelon the number of characteristic X-ray lines would be found to be as great as the number of light spectral lines are now known to be.

Many thanks are due to Professor Stewart and the Staff of the Physics Department of the University of Iowa where this work was performed for assistance in this research.

# *NOTE ON METHODS OF OBSERVING POTENTIAL DIFFERENCES INDUCED BY THE EARTH'S MAGNETIC FIELD IN AN INSULATED MOVING WIRE*

By Carl Barus and Maxwell Barus

Department of Physics, Brown University

Communicated January 24, 1918

In *Science,* **45,** 1917, p. 270, it was shown that the current induced in a rod about a meter long by the earth's vertical magnetic field, could be made surprisingly evident by the aid of an ordinary galvanometer, synchronized with the period of the inductor pendulum, of which the rod acts as the bob of a bifilar metallic suspension, including the galvanometer. Thus with a galvanometer showing but $10^{-6}$ amperes per cm. of deflection, and with a common period of about 4 seconds, the double amplitude of the spot of light reached over 20 cm. per average knot of speed of the rod. Similar results must be obtainable in case of a ship, where the charges accumulating at the ends of a long transverse rod may be tapped off into the water; or in case of a train where the tracks are available for dissipating charge. If, however, the rod is insulated, the determination of the potential difference in question is quite difficult. We made many attempts, as for instance the dissipation of charge from the ends by radium, electric incandescence, etc., the magnetic screening of parts of a circuit, etc., all to no practical effect. It seemed necessary therefore to carry the charge from end to end of the wire bodily; i.e., to reverse the principle of any type of induction electric machine, possibly with the additional object of securing intensification. Here also we have no ultimate success to record; but the secondary results are not devoid of interest, the idea being to construct, with precision, a large intensifier to pick up whatever electrostatic potentials may momentarily or permanently exist in the vicinity.

*Simple Apparatus.*—The apparatus (fig. 1), producing current is essentially cylindrical in shape, large and capable of revolving around an axle $a$ with considerable speed. The electrical parts are of light metal, all insulated or screwed firmly to insulating discs attached to the axle. This frame work will not be shown in the figures.

$A, A', A'', A'''$ are four insulated plates adjusted like the staves of a barrel, each making up about $\frac{1}{4}$ of its cylindrical area. Hence if the diameter of the barrel is $D$ and its length $L$ the area of each segment is $A = \pi LD/n$, if there are $n$ segments. The plates $A$, communicate by means of metallic rods $s, s', s'', s'''$, with the commutator segments or posts $c, c', c'', c'''$, each system, $A, s, c$, etc., being insulated.

When the barrel revolves, a pair of brushes $b\ b''$ serve to put the two segments $A\ A''$ momentarily in contact when in the horizontal position. A similar pair of brushes $b'$ $b'''$ put the segments $A'$ $A'''$ momentarily in contact when in the vertical position. These brushes lead to the inclosed sensitive galvanometer $G$. Hence there will be two contacts for each pair of segments $A$ for each rotation. $B$, $B'$ are auxiliary insulated armatures and here removed.

Let the earth's vertical field be $H$ and suppose the corresponding insulated metallic spokes $s$ $s''$ in contact through $b$ $b''$. Suppose the barrel to move forward as indicated at $v$ in the direction of its axis, i.e., out of the diagram. Then there will be an induced electric field $F$ in the direction $A''$ $s''$ $s$ $A$, due to the cutting of the earth's vertical field. The result of the momentary contact is thus a positive charge $q$ on $A$ and an equal negative charge on $A''$.

When as a result of the rotation of the drum around its axis, the segments reach the position $A'$ $A'''$ electromagnetic induction ceases and the field $F$ from this source is zero. Hence on momentary contact the charge will pass through the galvanometer $G$. With continued rotation this process is indefinitely repeated, so that a nearly continuous current flows through $G$; but there is no intensification.

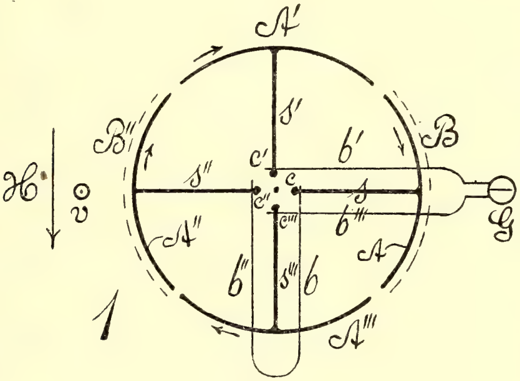


FIG. 1

*Elementary Estimate.*—Let the drum move in the direction of its axis with a speed $v$, so that the spokes $s + s'' = D$ cut the earth's vertical field $H$ at this rate. Hence if the difference of potential between the plates $A$ and $A''$ is $V$, when $c$ and $c''$ are in contact

$$V = HDv \text{ elm. units.} \tag{1}$$

Again when $c$ and $c''$ are insulated $A$ and $A''$ may be considered the plate of a condenser and so long as the field within may be considered as virtually uniform, we may estimate by Gauss's theorem

$$q = \frac{AV}{4\pi D} = \frac{A}{4\pi}\,\frac{Hv}{3 \times 10^{10}} \text{ electrostatic units} \tag{2}$$

where $q$ is the positive charge on the area $A$. Finally if the drum rotates around its axis $a$ with the speed of $N$ turns per second and if there are $n$ segments, $A$, the corresponding mean current passing through the galvanometer, with inclusion of the value of $A$ above, on reducing to practical units will be

$$I = NDLHv/3.6 \times 10^{20} \text{ amperes} \tag{3}$$

Let $N = 100$; $D = 100$ cm.; $L = 100$ cm.; $H = 0.4$ dynes per unit pole; $v = 50$ cm./sec. or about 1 knot. Then

$$I = \frac{10^2 \times 10^2 \times 10^2 \times .4 \times 50}{3.6 \times 10^{2D}} = 5 \times 10^{-14} \text{ amperes, nearly.}$$

i.e., the current traversing the galvanometer per knot of axial motion through the earth's vertical field would scarcely be perceptible by an extremely sensitive galvanometer.

In our trial of the apparatus we used a disc pattern like figure 3, putting both a galvanometer and a telephone in series, at $G$. On charging auxiliary armatures like $B$, $B''$ in figure 1, with a small Wimshurst machine, a loud rattle was immediately heard in the telephone and the galvanometer showed an average current of $2 \times 10^{-6}$ amperes at about 10 (or less) rotations per second. This slowly dropped owing to leakage of charge from the armatures; but at least $10^{-6}$ amperes persisted indefinitely. Discharging the armatures was followed with immediate permanent silence in the telephone. If we estimate the difference of potentials of the armatures as $10^4$ volts, and the earth's field $H$ (as above) to supply $5 \times 10^{-5}$ volts, the latter should produce $5 \times 10^{-5}$ $(2 \times 10^{-6}/10^4) = 10^{-14}$ amperes, which is of the order of values computed. Charging the armatures with 25 volts from a storage battery produced no result, as was to be expected.

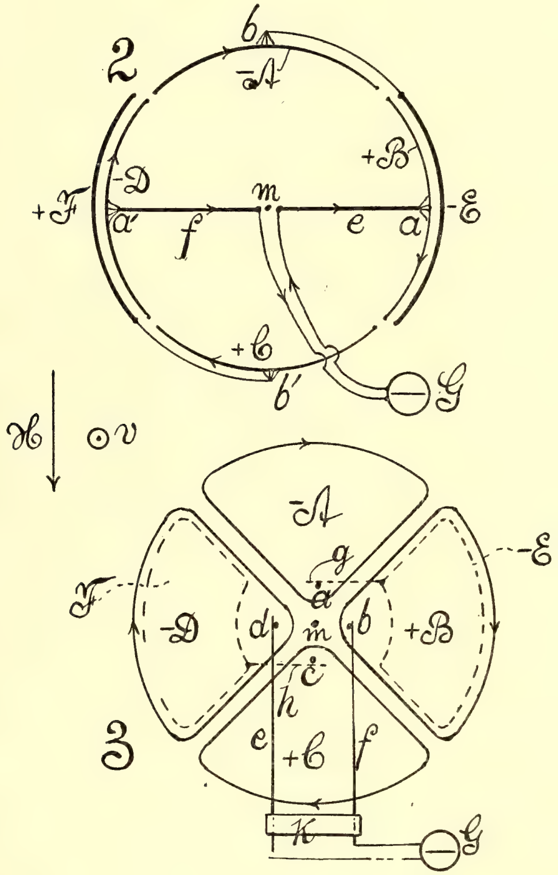
*Intensification.*—The drum was now to be modified as suggested in figure 2, though the experiments below were carried out with the disc form, figure 3, which is more easily constructed. All contacts must be synchronous and momentary.

In the disc type of machine (fig. 3) (as here constructed about 2 feet in diameter), $A$, $B$, $C$, $D$ (in front) are the segments, rotating around the axis $m$, and $E$, $F$ the armatures behind the diagram. The metallic posts or commutators $a$, $b$, $c$, $d$ stick out normally from the disc both in front and behind it. The vertical brushes $d$ and $b$ touching diametrically opposite posts in front successively, include the galvanometer, etc., $G$, $K$ being an insulating holder. The horizontal brushes, $g$ and $h$, in metallic connection with $E$ and $F$, respectively, touch the other two posts successively behind the disc and store the charges of $A$ and $C$ on $E$ and $F$. $H$ shows the earth's magnetic field and $v$ the motion through it.

If we suppose that any charge on the armature $E$, for instance, induces the same but opposite charge on the segment $B$, the charges successively passing through the galvanometer for each quarter turn may be scheduled in such a way that the sum is a geometrical progression. Thus for $N$ turns $Q = \Sigma q = (4/5)k^N q$ coulombs, nearly, where $k$ is a large empiric constant depending on the method of initial charging and on the effective capacity of the armature-quadrant condenser.

Finally if there are $N$ turns per second and $n = 4$ segments, the average current is in the first second:

$$I = k^N DLHv/1.8 \times 10^{21} \text{ amperes.}$$



FIGS. 2 AND 3

Thus even if $N = 10$ turns for 1 second, 20 turns for 2 seconds, etc., the current would soon be appreciable.

*Observations with the Intensifier.*—The telephone and galvanometer may be inserted together and their indications as to intensity are identical; but the galvanometer additionally shows the sign of the current. The average current of $10^{-7}$ amperes could still be easily heard in the telephone. The electroscope and electrometer were also tested; but these instruments are unsuitable as on open circuit the potential may suddenly rise to sparking values and ruin them.

Great care was taken to secure all insulators *firmly* and to allow only metal appurtenances (brushes) to touch each other.

The sounds (taps) were usually strong on starting the intensifier. As the speed increases to about 5 or 10 rotations per second a maximum of current was reached. This may be an average current of $\pm 10^{-5}$ amperes or more, as specified. The current frequently has an approximately regular period of reversal; as for instance (*L*, low current, here about $-2.5 \times 10^{-6}$ amperes; *H*, high current, $+5 \times 10^{-6}$ ampere, both fluctuating).

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Successive currents............. | L | H | L | H | L | H | L | H | L | H | L | H, etc. |
| Period of each (seconds)........ | 25 | 30 | 30 | 30 | 30 | 35 | 45 | 35 | 40 | 35 | 45 | 35 etc. |

The current is zero when the armatures are metallically connected. This shows that stray magnetic lines from the motor are without *direct* effect, a conclusion which was further tested by supplying stationary looped magnetic lines from a strong horseshoe magnet. Left to itself the machine picks up potentials either from within itself, incidentally, or from the room or surrounding walls in cold weather; and these are much higher than any which can be produced with a moving magnetic field. The machine, thoroughly flame-cleaned on both sides, with armatures in contact, soon charges itself, the maximum potentials being usually approached by successive alternations of rapidly increasing amplitude. Finally sparks appear at the brushes. If a bunsen flame is near, its depolarizing effect is appreciable at about a foot; the machine ceases to function at about six inches. Only under favorable conditions were we able to build up potentials from the induction of a small horseshoe magnet. It is obvious therefore that before putting such an apparatus to the final test it will have to be constructed with precision and experiments made in an environment free from stray potentials, with elimination of voltaic potentials, etc., in the parts, as everything of this nature is so quickly assimilated and intensified. As to the means of rating the value of the inducing potential, the current obtained on disconnecting the armatures for a definite small interval of time suggests itself.

The efficiency is enormously increased by diminishing the distance between the armatures and the rotating segments (condenser) though in the above improvised apparatus it was not possible to approach closer than an average half inch.

# DEPENDENCE OF THE SPECTRAL RELATION OF DOUBLE STARS UPON DISTANCE

By C. D. Perrine

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Communicated by E. B. Frost, January 7, 1918

In the course of an investigation of the cause of the spectral differences of the stars, an examination of the spectra of the components of double stars was made. Almost at once it was seen that there was a relation other than the well known conclusion that in contrasted pairs the fainter component is generally of the earlier type. It was found that such stars are almost invariably very distant, and that the reverse generally occurs in the near pairs.

The principal data used in this investigation were taken from the observations of the spectra of 745 double stars made at Harvard and classified by Miss Cannon.[1] Seventy-eight stars were selected of which the spectra of both components had been determined, having a difference of brightness of at least half a magnitude and known to be either binary or to have common proper motion.

It was noticed that the stars in which the fainter component was of the earlier type were, in general, closer together than those in which the companion was of a later type. The effect of distance on the separations of these stars was investigated by means of proper motion. The result indicated a greater average absolute separation in both classes than for the stars whose magnitudes and spectral classes are nearly the same. Incidentally, however, it was shown that the pairs in which the fainter component is of earlier type are distant, whereas those in which the fainter companion is of later type, with the exception of the stars both of whose components are of classes B and A, are much nearer.

Of the stars selected, 24 have the fainter components of the same spectral type, 26 have the fainter components of later and 28 of earlier spectral type. The last two groups are given in detail in the tables following.

With the exception of the pairs (12 in number) of table 1, which have *both components of classes B and A* and whose proper motions are less than 0″.05, the remainder show large differences of spectral type and large average proper motion. It is to be noted that 4 of the 12 stars with small $\mu$ in this table, having the fainter component of *later* type, belong to 15 Cephei. The consistency in the spectral relations of this group is noticeable.

The pairs of table 2, with two exceptions, show small proper motions. The proper motions of these two stars cannot be said to be large and it is further to be noted that their differences of spectral type are rather small. The average $\mu$ of the entire 28 stars is but 0″.037, or omitting the largest two, is but 0″.030. The criterion of proper motion, therefore, indicates that these stars are at the same general distance as the class B stars.

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[1] *Ann. Harvard Coll. Obs., Cambridge,* **56,** no. 7.

The stars of table 2 also show a decided preference for the galaxy, half of them being within 15° and three-quarters of them within 40° of the galactic plane. This is significant when it is considered that the principal stars of these pairs belong almost entirely to the middle and later types of spectra.

TABLE 1

FAINTER COMPONENT OF LATER TYPE

| MAGNITUDE BRIGHTER COMPONENT | Δ MAGNITUDE | SPECTRAL CLASS | | $\mu$ | S |
|---|---|---|---|---|---|
| | | Brighter | Fainter | | |
| 5.6 | 0.9 | $A_5$ | $G_5$ | 0″.143 | 23″.7 |
| 5.2 | 2.6 | $F_5$ | G | .295 | 49.4 |
| 6.0 | 0.7 | $A_0$ | $A_2$ | 25 | 16.6 |
| 3.0 | 3.3 | $B_5$ | $A_0$ | 52 | 117.3 |
| 4.9 | 0.6 | $B_8$ | $A_0$ | 82 | 7.8 |
| 1.3 | 6.3 | $B_8$ | G | .247 | 176.7 |
| 5.3 | 1.8 | $F_0$ | $G_0$ | .289 | 288.1 |
| 5.4 | 1.4 | $A_0$p | $A_3$ | 29 | 145.3 |
| 0.3 | 1.4 | $G_0$ | $K_5$ | 3.67 | 21.9 |
| 2.9 | 2.2 | $A_2$ | $F_5$ | .131 | 231.0 |
| 4.5 | 2.2 | $F_0$ | $K_0$ | .169 | 108.3 |
| 4.3 | 2.2 | $B_3$ | A | 34 | 41.3 |
| 4.9 | 2.7 | $A_2$ | F | 65 | 88.8 |
| 6.0 | 0.6 | $B_8$ | A | 38 | 21.7 |
| 5.8 | 0.8 | $B_3$ | A | 22 | 35.7 |
| 4.6 | 1.9 | $A_3$ | G? | .230 | 6.9 |
| 5.8 | 0.7 | $B_3$ | $B_5$ | 18 | 22.4 |
| 3.6 | 2.5 | $A_5$ | $K_0$ | .107 | 337.1 |
| 4.8 | 2.7 | $B_5$ | A | 10 | 22.8 |
| Var. 3.4 | 4.4 | $B_2$ | $B_3$ | 8 | 46.0 |
| 6.7 | 0.9 | $B_0$ | $B_9$ | 13 | 183.4 |
| 6.8 | 0.8 | $B_5$ | $B_9$ | 13 | 136.1 |
| 6.7 | 0.1 | $B_0$ | $B_5$ | 13 | 236.3 |
| 6.8 | 1.0 | $B_5$ | $B_9$ | 13 | 192.4 |
| 4.2 | 1.7 | $B_9$ | $A_0$ | .121 | 26.9 |
| 4.8 | 4.8 | Ap | K | .138 | 222.8 |

Further evidence is found in 19 of the Harvard stars which have composite spectra and in 62 stars of Campbell's Catalog of Spectroscopic Binaries,[2] in which the spectra of both components have been observed or strongly suspected.

For both spectra to appear on a photograph, the difference in brightness of the components of close binary stars will usually be small. On account of the tendency for components of nearly the same brightness to have similar spectra, the evidence from these two sources is not of as great weight as the preceding. It is, nevertheless, quite definite.

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[2] *Lick. Obs. Bull., Berkeley*, **6**, 1910, (46).

Of the 19 Harvard composite stars, two are somewhat uncertain as to the difference of spectral class, both being of early type. Of the remainder, the one having the largest proper motion (0″.15) has the fainter component of the later type. The remaining 16 all have very small proper motions. Of these, 5 have the fainter component of later type and 11 of earlier type. All of the most strongly marked spectral contrasts have the fainter components of earlier type and all but one of these 11 are well marked in respect to spectral contrasts.

TABLE 2

Fainter Component of Earlier Type

| MAGNITUDE BRIGHTER COMPONENT | Δ MAGNITUDE | SPECTRAL CLASS | | $\mu$ | S |
|---|---|---|---|---|---|
| | | Brighter | Fainter | | |
| 6.2 | 1.2 | $G_0$ | A | 0″.018 | 2″.9 |
| 4.3 | 0.9 | $A_3$ | A | 42 | 2.6 |
| 2.3 | 2.8 | $K_0$ | A | 70 | 10.7 |
| 5.0 | 1.4 | $G_5$ | B? | 34 | 6.9 |
| 5.5 | 1.2 | G | $A_3$ | 29 | 3.0 |
| 5.9 | 1.1 | $A_0$ | $B_9$ | 27 | 28.9 |
| 5.8 | 1.2 | G | A | 47 | 0.4 |
| 5.4 | 3.1 | K | A | 7 | 2.3 |
| 3.9 | 1.9 | $K_0$ | G | 97 | 13.3 |
| 6.4 | 0.5 | G | $A_2$ | 41 | 63.4 |
| 4.2 | 2.4 | $G_5$ | $A_2$ | 54 | 30.6 |
| 5.2 | 1.5 | $K_0$ | $A_3$ | 17 | 20.6 |
| 5.3 | 0.5 | $A_2$ | A | 33 | 21.6 |
| 2.7 | 2.4 | $K_0$ | A | 49 | 2.8 |
| 3.5 | 4.5 | $K_0$ | $G_0$ | 155 | 104.8 |
| 1.2 | 5.8 | Ma | A | 34 | 3.2 |
| 6.4 | 1.1 | K | $F_5$ | 48 | 111.2 |
| 6.1 | 1.7 | G | A | 11 | 45.5 |
| 5.6 | 1.2 | $F_5$ | A | 13 | 0.6 |
| 7.1 | 1.0 | G | A | 42 | 4.1 |
| 6.0 | 1.5 | $F_5$ | $A_2$ | 18 | 15.0 |
| 3.2 | 2.1 | K | $B_9$ | 9 | 34.7 |
| 5.6 | 3.3 | $K_0$ | A | 34 | 19.8 |
| 6.5 | 1.1 | F | A | Small | 1.2 |
| 5.7 | 2.2 | $G_0$ | A | 20 | 89.9 |
| 6.1 | 0. | $G_5$ | $A_2$ | 25 | 23.0 |
| 3.9 | 3.2 | K | $B_8$ | 2 | 107.1 |
| 3.3 | 2.8 | $G_0$ | $A_0$ | 28 | 205.2 |

Of the 62 stars of Campbell's Catalog, 30 belong to types O and B and 23 to type A. It is reasonably certain, therefore, that all of the O and B stars are distant and the probability is strong that most of the 23 stars of type A are distant also. Campbell concludes,[3] I understand, that the rule that when one spectrum is considerably fainter than the other, the spectrum of the secondary is apparently of a slightly earlier type than the spectrum of the primary, applies to these stars, which we have just found to be distant.

Both of these pieces of evidence are confirmatory of the condition previously found.

I have also examined several well known systems whose colors have been observed, viz., $\alpha$ Herculis, $\eta$ Geminorum, $\gamma$ Delphini, $\eta$ Cassiopaeiae, $\xi$ Boötis, $\beta$ Cephei and $\gamma$ Leonis. These stars also show in general the same relation to distance.

Careful consideration of the data seems to show beyond doubt that the relation is to distance coupled with low galactic latitude. It cannot well depend upon the actual separations of the stars, for the ranges in that respect seem to be about the same in all of the groups. It can scarcely depend upon the differences of mass of the conponents as indicated by their differences of brightness, for a similar reason. There is a difference of absolute magnitude between the two groups, but there seems to be no reason for suspecting a relation in this case, whatever may be the bearing of such differences in others.

It is not known whether both components of double stars were originally of the same spectral type or not. Investigators have generally assumed that they were. It is not possible to say, therefore, just what the course of change has been. However, we may justly conclude, I think, that the conditions are such in these regions as to produce *opposite* spectral effects in the components. There can be little doubt that the fainter components are in general also of smaller mass. The conclusion may be stated, therefore, in the following form:—The conditions appear to be such that if two stars of unequal mass were introduced into the near region the smaller body would move more rapidly toward the later stage than the larger one, whereas in the relatively distant galactic regions the tendency would be for the smaller body to become of *earlier* type more rapidly than the larger one.

This investigation also shows that the greatest differences in spectral type are in general found among the stars whose components show the greatest differences in brightness. The differences of brightness in the pairs having the same spectra are consistently small for all types.

The results of this investigation seem to indicate with considerable force that some external cause is operating in more or less definite regions of our stellar system upon the conditions which produce spectral class.

The details have been given more fully in a paper which will be published in the *Astrophysical Journal.*

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[3] *Ibid.*, 6, 1910, (47).

# HYPOTHESIS TO ACCOUNT FOR THE SPECTRAL CONDITIONS OF THE STARS

By C. D. Perrine

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Communicated by E. B. Frost, January 7, 1918

The peculiar behavior of double stars in the near and relatively distant regions of the galaxy, which has just been discovered (these Proceedings, **4,** 1918, 71), together with the well known preference of the stars of type B for the Milky Way and the general preference of the later types for the nearer regions of space, suggest the conclusion that spectral class depends largely upon external causes.

Further study of the brightnesses and spectra shows that there is a strong similarity between the brighter stars of early type if arranged in the order B, O, gaseous nebulae, and the changes in the novae in their early stages. As is well known, all of these objects are confined to relatively distant regions in the direction of the galaxy. There is also reason to think that the same cause which is believed to underlie the phenomena of the outbursts in the novae may be a vital factor in the determination of spectral class among the ordinary stars.

As a result of these investigations the following general hypothesis has been formulated to account for the present classes of stellar spectra.

*Hypothesis.*—The cause is dual, depending upon the amount of cosmic matter and upon phenomena of radiation and condensation. Many of the A stars, the B and O stars, the planetary and irregular gaseous nebulae, the novae and perhaps the Cepheid variables, are confined to the galaxy because there the matter is sufficiently plentiful to cause an increase of energy, the energy from the matter swept up being in excess of that lost by radiation. The direction of spectral change under such conditions in *toward the nebulae.*

In the regions (distant or near) where there is little or no cosmical matter, radiation will be in excess of the energy received from external sources and the direction of change will be *toward the late types.*

In a considerable portion of the system the changes of spectral class may be due simply to retardation.

The hypothesis may be further generalized as follows—

The spectral condition of a star depends chiefly upon its size and mass and the external conditions of density of cosmical matter and relative velocities of star and matter.

Upon this hypothesis the stars are probably all pursuing one definite course of very slow change toward extinction, but each individual star will be pursuing a course which may have many whole or partial cycles due to varying external causes.

Details of this investigation are given in a paper which has been sent to the *Astrophysical Journal.*

# NATIONAL RESEARCH COUNCIL

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## MEETINGS OF THE EXECUTIVE COMMITTEE

### Thirty-fourth Meeting—December 4, 1917

The meeting convened in the offices of the Council in the Munsey Building, Washington, D. C., and was called to order at 9.15 a.m. by the Chairman, Mr. Hale.

Messrs. Marston T. Bogart, Russell H. Chittenden, George E. Hale, Arthur A. Noyes, Raymond Pearl, S. W. Stratton, Charles D. Walcott, and, by invitation, W. F. Durand and Charles E. Mendenhall were present.

The minutes of the meeting of the Committee of November 10 were approved as prepared and submitted to the members of the Committee.

The Chairman of the Council reported:

1. That Col. P. D. Lockridge, Acting President of the Army War College, and Capt. Roger Welles, Director of Naval Intelligence, U.S.N., have been appointed members of the Council and of its Military Committee.

2. That at a recent meeting of the Military Committee the request of the British Ministry of Munitions, and a later communication from the British War Mission, relative to the organization of an experimental liaison were considered, involving the appointment of a competent group of American scientists to secure adequate intercommunication between English and American industrial and military projects and development. This request was cordially received and approved, having already received the approval of the Executive Committee. A full discussion of the subject at meetings of the Military Committee, however, led also to the approval of the organization of a more extensive program, involving intercommunication between the entente allies on all subjects of industrial and scientific importance. A subcommittee, consisting of the Director of the Bureau of Standards, the Acting President of the Army War College, and the Director of Naval Intelligence, was appointed to submit recommendations with regard to the procedure necessary to establish this liaison, under direct authority of the War and Navy Departments of the Government.

A financial statement, showing expenditures since the last monthly meeting of the Committee, was submitted and approved.

Upon recommendation of the Vice-Chairman of the Engineering Committee, the following were added to the membership of this Committee:

Comfort A. Adams, Harvard University; Jesse Coates, Consulting Engineer.

Mr. Durand reported for the special committee, consisting of himself and Messrs. Millikan and Stratton, which was appointed to undertake the selection of a larger committee to consider the organization and various problems of the Patent Office. As a result of discussion the following members of such a committee were appointed:

Leo H. Baekeland, William F. Durand, Thomas Ewing, Frederick P. Fish, Robert A. Millikan, Edwin J. Prindle, Michael I. Pupin, S. W. Stratton.

Upon recommendation of the Chairman of the Chemistry Committee, Lieut. Col. Wm. H. Walker was added to the membership of this Committee and Frederick W. Brown, of the Bureau of Soils of the U. S. Department of Agriculture, was named a member of the Subcommittee on Chemistry of Soils and Fertilizers.

The Chairman of the Chemistry Committee also reported that a donation of $100 a month had been received from B. T. Bush of the Antoine Chiris Company of New York to assist the work of this Committee. Upon motion, the Executive Committee tendered a vote of thanks to Mr. Bush for his cooperation and financial assistance in this connection.

The Chairman of the Council reported upon conferences which had been held relative to the organization and work of the proposed Forestry Committee and submitted for consideration a list of persons who might be favorably considered for appointment as members of the Committee. After discussion it was voted that a special committee, consisting of Irving W. Bailey, Raymond Pearl and Karl F. Kellerman, be appointed to consider the organization and duties of the Forestry Committee of the Council as a correlating agency for research in this field and to submit to the Executive Committee recommendations with regard to the personnel of the Forestry Committee, the proposed scope and functions of this Committee, and its status in the general organization scheme now under consideration relative to the future activities of the Council.

Mr. Stratton, for the Subcommittee of the Military Committee, appointed to submit recommendations with regard to procedure necessary to establish a scientific liaison between the entente allies, submitted a report with recommendations which had been adopted by the Military Committee at its meeting of December 3. These recommendations were considered carefully by the Executive Committee and, after certain minor modifications, were adopted as follows:

> In order to facilitate the securing and dissemination of scientific and technical industrial information for military purposes, the Military Committee of the Research Council recommends as follows:
>
> 1. There shall be a committee known as the Research Information Committee, to consist of the Chief of the Military Intelligence Section of the Army, the Director of the Naval Intelligence Bureau of the Navy, and a representative of the National Research Council to be nominated by the Chairman of that body and appointed by the Council of National Defense, who shall be Chairman of the Committee.
>
> 2. A scientific and technical industrial representative of the National Research Council shall be attached to the offices of both the Naval and Military Intelligence in London, Paris, and such other countries as may be considered desirable, and the said representative together with the said military and naval attachés shall form a committee at each station acting as a subcommittee of the Research Information Committee.
>
> 3. The duties of this Committee shall be to provide for the collection of scientific and technical industrial information relating to military and naval affairs at home and abroad and for the dissemination of the same to the Governmental departments interested both at home and abroad.
>
> 4. This Committee shall be provided with a permanent secretary, and with the clerical

and office facilities requisite for such files, indices and correspondence as the Committee may direct.

5. In addition to the usual duties in connection with the keeping of files, indices, conduct of correspondence, and so forth, the especial duty of the Chairman of the Committee shall be to assist the proper officials in securing, locating, and disseminating the information required, under such rules and regulations as the Committee may formulate from time to time.

6. When scientific and technical industrial information is required which involves investigation to be made in coöperation with foreign civil authorities or military authorities not at the front, the investigators selected shall be duly vouched for by this Committee and shall be accredited by it to the Research Information Committee abroad.

Upon motion of Mr. Noyes it was voted that these recommendations be transmitted to the Council of National Defense with such additional details relating to the cost and organization of the proposed liaison as may be formulated by the subcommittee of the Military Committee in consultation with the Chairman of the Council.

The Chairman of the Council reported that the Committee on Organization had tentatively considered a scheme for the organization and future development of the Council, copies of which were laid before members of the Executive Committee. Extended discussion took place, after which it was decided to hold another meeting of the Executive Committee in the near future to continue such consideration.

The Chairman of the Council was requested to appoint a special committee to consider the acquisition of adequate office accommodations in Washington.

The meeting adjourned at 1.30 p.m.

### Thirty-fifth Meeting, December 19, 1917

The meeting convened in the offices of the Council, Munsey Building, Washington, D. C., and was called to order at 9.15 a.m. by the Chairman of the Council, Mr. Hale.

Marston T. Bogert, Russell H. Chittenden, Edwin G. Conklin, Gano Dunn, George E. Hale, Van H. Manning, R. A. Millikan, Arthur A. Noyes, Raymond Pearl, S. W. Stratton, Victor C. Vaughan, Charles D. Walcott, and, by invitation, William F. Durand and John C. Merriam were present.

The Chairman of the Council reported:

1. That arrangements have been made to lease the building at 1023 16th Street, Washington, D. C., for the offices of the Council, at a rental of $500 a month, and that a statement of estimated office expenses of the Council, aggregating $29,250.00, for the year 1918, has been submitted to the Director of the Council of National Defense.

2. That the resignation of Mr. Mendenhall as Chairman of the Committee on Relations with State Research Councils has been accepted and that Mr. Merriam, of the University of California, has been appointed Chairman of this Committee.

Upon suggestion of the Chairman, Mr. Merriam reported upon the proposed scope and organization of the Committee on Relations with State Research Councils. Upon motion the Committee on Relations with State

Research Councils was requested to submit a report to the Executive Committee as soon as practicable, with recommendations concerning steps to be taken to affiliate and closely coordinate the work of State Research Councils with that of the National Research Council.

In extension of the discussion which took place at the meeting of the Executive Committee on December 4 relative to the reorganization and future development of the Council, Mr. Hale spoke of the rapid extension of its work and particularly of the importance of its Military Committee and of its governmental associations, as well as of the necessity of basing any future scheme of organization on the charter of the National Academy of Sciences, as an official advisor of the Government.

A proposed plan of organization was laid before the Committee and tentatively adopted as a basis for further consideration and discussion.

Mr. Dunn explained that the Engineering Foundation had expressed its willingness and desire to furnish financial assistance in aid of researches which might advisedly be undertaken by the National Research Council in the field of engineering, and upon his suggestion, as Chairman of the Engineering Committee of the Council, it was voted that the Executive Committee request the Engineering Committee to submit suggestions to the Engineering Foundation with regard to financial needs for problems in engineering research.

The meeting, which adjourned at 11.40 a.m., was followed by two sessions of a joint meeting of the Executive Committee of the Research Council and of the Council of the National Academy of Sciences, at which discussion took place with regard to the proposed organization of the Research Council and its relations to the National Academy of Sciences.

### THIRTY-SIXTH MEETING, JANUARY 17, 1918

A joint meeting of the Council of the National Academy of Sciences and of the Executive Committee of the National Research Council convened at 9.30 a.m. in the offices of the Research Council at 1023 16th Street, Washington, D. C., with President Walcott of the National Academy of Sciences in the chair.

Present: Members of Council of National Academy of Sciences: Russell H. Chittenden, Whitman Cross, George E. Hale, William H. Howell, Charles D. Walcott. Members of Executive Committee of National Research Council: Marston T. Bogert, Russell H. Chittenden, George E. Hale, Van H. Manning, Robert A. Millikan, S. W. Stratton, Charles D. Walcott, William H. Welch. Also present by invitation: William F. Durand, Simon Flexner, Charles E. Mendenhall, John C. Merriam, Robert M. Yerkes, and William H. Walker.

The Chairman of the Council reported:

1. That an allotment of $29,250 has been authorized, by the President of the United States, from the emergency appropriation at his disposal to apply toward the estimated office expenses of the National Research Council for the year 1918.

2. That the building at 1023 16th Street has been rented by the Research Council, and that the twenty-two rooms in the building have already been assigned to provide for the growing activities of the Council.

3. That in accordance with the action of the Executive Committee at its meeting on December 4, the proposed recommendations with regard to the organization of a Research Information Committee have been submitted to the Council of National Defense. These recommendations were approved, and authority issued for the establishment of three committees to inaugurate the proposed work. The Committee in the United States consist of S. W. Stratton, Chairman, representing the National Research Council; Captain Roger Welles, Director of Naval Intelligence, U. S. N.; and Colonel Ralph H. Van Deeman, Chief of Military Intelligence Section, Army War College. The London Committee will consist of Henry A. Bumstead of Yale University, representing the National Research Council, together with representatives of the Army and Navy Intelligence Services to be appointed; and the Paris Committee will consist of William F. Durand, Vice Chairman of the Engineering Committee of the National Research Council, representing the Council, with representatives of the Army and Navy Intelligence Services to be appointed. The State Department has agreed to appoint Mr. Bumstead and Mr. Durand as Scientific Attachés of the Embassies at London and Paris respectively. Mr. Harold D. Babcock, a physicist at the Mt. Wilson Solar Observatory, has been appointed Technical Assistant of the Washington Committee, and it is expected that he will report for duty on January 21, with offices in the National Research Council building. A request has been made of the Council of National Defense for funds to provide for the expenses of these three offices. Mr. K. T. Compton of Princeton University has been appointed Technical Assistant of the Paris committee.

The resignation of Cary T. Hutchinson as Secretary of the National Research Council was presented and accepted,

The resignation of Raymond Pearl as Chairman of the Agriculture Committee of the Council was presented and accepted.

Upon nomination of the Chairman of the Council, John Johnston, at present in charge of the offensive gas warfare work of the U. S. Bureau of Mines, was elected Executive Secretary of the National Research Council at an annual salary of $5000, to take effect as soon as Mr. Johnston is able to accept the appointment and devote his entire time to the work of the Council.

Upon recommendation of the Chairman of the Chemistry Committee, the following subcommittee was appointed with Chairman as indicated:

Sub-Committee on Chemical Engineering: Chairman, Charles F. McKenna, 50 Church Street, New York City.

The appointment of the Sub-Committee on Chemical Engineering was requested by the American Institute of Chemical Engineers, and the Chairman was nominated by the Institute.

The Chairman of the Chemistry Committee also reported that:

William J. Comstock, formerly of the Chemical Staff of Yale University, has come to Washington to aid in the work of the Chemistry Committee, to which he will devote his entire time, and that

The Chairman of the Chemistry Committee has been made a member of an Advisory Committee of the War Trade Board.

Upon nomination of the Chairman of the Council, Charles B. Davenport was appointed Vice-Chairman of the Anthropology Committee.

Mr. Merriam, Chairman of the Committee on Relations with State Research Committees, presented the following report:

It is the opinion of your committee that the greater part of the problem work of the National Research Council can be handled most satisfactorily through the special committees or divisions established for research in specific fields of investigation. To a limited extent phases of these investigations of problems may be turned over to adequately organized and supported research groups in different parts of the country, some of these groups representing educational or research institutions and others being organized under the auspices of state governments or state Councils of Defense.

As numerous important problems of research relate to the development of natural resources, industries, consideration of health conditions, and other problems of a local nature, it seems desirable in many cases to have an organization of research interests related to the state, and if possible supported by it. Such organizations might, under existing conditions, be best cared for as committees under the state Councils of Defense.

It is desirable to have the National Research Council so related to the state research scientific committees that the results of their investigation may become quickly available to the central office of the Council, and that the needs of the Council for work of a local character may be met by the state committees.

Your Committee respectfully suggests the following definition of function, organization, and relations of State Research Committees:

Function of State Research Committees. Problems involving local needs in the development of natural resources, local industries, health conditions, or any matters to which science may lend its aid; and problems involving local materials, industries, laboratories or talent, development or use of which would contribute to the good of the nation as a whole or in part, regardless of questions of needs of the state in which the investigation originates.

Organization of State Committees. In initial organization the state committee should be small, but widely representative of fields of research. Additions to membership should be made as actual research progresses. The committees should, at the outset, include members of state boards covering work in which scientific research plays an important part, and representatives of scientific organizations or institutions at which significant research is in progress, especially institutions in which research committees are organized.

State research committees should at this time be organized as sub-divisions of state Councils of Defense, where such councils exist.

Financial support of state committees should come from state funds received by way of the state Councils of Defense, or from other special funds. It is in all cases desirable to have secretarial organization permitting full correspondence on all matters relating to the committee.

Relation of Research Council to State Committees. It is desirable to have the state research committees affiliated with the National Research Council; the results of work of these committees should be reported to the state Council of Defense and to the National Research Council by way of either or both of these bodies; results obtained by the state research committees should go to the Council of National Defense when needed.

It was voted that the above report be adopted as furnishing the mode of procedure to be used by the National Research Council in directing its relations to state research committees.

The Chairman of the Council spoke at length with respect to the adoption of an organization of the Council devised to meet immediate war needs. The present organization, adopted in 1916 for a period of one year, is essentially a peace organization. Under war conditions the Council is engaged almost exclusively in scientific and research work for the Army, Navy, War Industries Board, and other Government bodies, and the wide distribution of the members of its committees has been a source of inconvenience and sometimes of confusion. Such a war organization would involve the concentration in Washington of comparatively few men representing the most essential work.

For convenience of operation, it is proposed that closely related subjects be grouped in Divisions, to avoid such difficulties as have sometimes arisen from the independent work of committees covering similar fields. The Executive Committee of each Division is, in general, to be composed of the smallest possible number of representative men, living in Washington or closely in touch with the work of the Government.

All of the old committees of the Council will remain in existence, to be called upon for such work as the various Divisions may assign to them.

He then submitted to the members present a schedule of the proposed war organization of the Council, which after discussion and upon motion was adopted, as follows:

### WAR ORGANIZATION OF THE NATIONAL RESEARCH COUNCIL

General Officers: Chairmen, Three Vice-Chairmen, Executive Secretary, Treasurer, Assistant Secretary.

Executive Board: General Officers, the Chairmen and Vice-Chairmen of the eight Divisions, the Chairmen of the five Sections of the Administrative Division, and ——— members at large.

1. Administrative Division: Sections on Foreign Relations; Governmental Relations; Relations with State Research Committees; Industrial Relations; Educational Relations.
2. Military Division.
3. Engineering Division.
4. Division of Physics, Mathematics, Astronomy, and Geophysics.
5. Division of Chemistry and Chemical Technology.
6. Division of Geology, Mineralogy, and Geography.
7. Division of Medicine, Hygiene, Surgery, Anatomy, Anthropology, Physiology, Psychology, and Zoology.
8. Division of Agriculture, Forestry, Botany, and Fisheries.

In adopting this plan, it was recognized that part of the work in Zoology would fall within Division 8.

It was also voted that an Executive Committee of the Executive Board be appointed to consist of the General Officers of the Council and of the Chairmen of the Sections of the Administrative Division to serve between meetings of the Executive Board for the consideration of current business.

Upon motion, the present officers of the Council were nominated and elected as General Officers of the War Organization of the Council as follows:

Chairman, George E. Hale.

First Vice-Chairman, Charles D. Walcott.

Second Vice-Chairman, Gano Dunn.

Third Vice-Chairman, Robert A. Millikan.

Upon motion of Mr. Hale the following additional officers were elected:

Executive Secretary, John Johnston.

Treasurer, Witman Cross.

Assistant Secretary, Walter M. Gilbert.

The treasurer was authorized to sign checks drawn on the bank accounts of the Council in the Riggs National Bank and with the National Academy of Sciences.

Upon further motion the following chairmen for sections of the Administrative Division were nominated and elected:

Section on Foreign Relations, S. W. Stratton.

Section on Relations with State Research Committee, J. C. Merriam.

Section on Industrial Relations, John Johnston.

Upon motion of Mr. Bogert, the Executive Committee of the Administrative Division was requested to serve as a nominating committee for positions of Chairmen and Vice-Chairmen of the other Divisions and Sections of the War Organization. Subsequently, Mr. Hale, as spokesman for the nominating committee, submitted recommendations resulting in the election of the following persons to be the positions named:

Military Division, C. D. Walcott, Chairman; S. W. Stratton, Vice-Chairman.

Division of Physics, Mathematics, Astronomy and Geophysics, R. A. Millikan, Chairman.

Division of Chemistry and Chemical Technology, M. T. Bogert, Chairman; W. H. Walker, Vice-Chairman.

Division of Geology, Mineralogy and Geography, J. C. Merriam, Chairman; Whitman Cross, Vice-Chairman.

Division of Medicine, Hygiene, Surgery, Anatomy, Anthropology, Physiology, Psychology and Zoology, Simon Flexner, Chairman.

The Executive Committee of the Administrative Division was also given authority to make such further appointments as may be necessary to positions of Chairmen and Vice-Chairmen of Divisions and Sections of the War Organization, and to determine the number of members at large to be appointed on the Executive Board.

Mr. Hale explained that preliminary work of organizations had already been done in connection with the Divisions of Chemistry and Chemical Technology, of Geology, and of Medicine. Upon his recommendation, an Executive Committee of the Division of Chemistry and Chemical Technology was appointed to consist of the following persons: Marston T. Bogert, John Johnston, Arthur A. Noyes and William H. Walker.

He also explained that it is the purpose of the Division of Medicine to correlate all of the medical research work bearing on the war, and that to this end cooperative plans had been entered into through the Surgeon-General's office of the Army and other military and civil research agencies.

Upon invitation, Mr. Merriam outlined the proposed work of the Division of Geology, Mineralogy and Geography.

Mr. Hale emphasized the importance of undertaking at present the promotion of industrial research in preparation for post bellum conditions and activities. Upon his recommendation authorization was given for the appointment of an Advisory Committee for the Section on Industrial Relations of the Administrative Division of the Council, to consist of prominent leaders of industry in the United States.

Inasmuch as the former membership of the National Research Council is still maintained, it was decided, upon nomination of the Chairman of the Council to request the President of the National Academy of Sciences to appoint the following gentlemen, who will have an active part in the work, as additional members of the Council: Henry A. Bumstead, Director, Sloane Physical Laboratory, Yale University; Whitman Cross, Geologist, U. S. Geological Survey; Charles B. Davenport, Director, Department of Experimental Evolution, Carnegie Institution of Washington; Frank W. DeWolf, State Geologist of Illinois; Douglas W. Johnson, Professor of Physiography, Columbia University; John Johnston, Research Department, American Zinc Lead, and Smelting Company; Philip S. Smith, Administrative Geologist, U. S. Geological Survey; Colonel Ralph H. VanDeeman, Chief of Military Intelligence Section, Army War College; Lt. Col. William H. Walker, Assistant Director of the Chemical Service of the National Army.

Mr. Stratton explained the proposed activities of the Research Information Committee recently organized, and emphasized the need for the collection of information by the Research Council with regard to scientific activities and progress in the United States and abroad bearing upon the war.

Upon invitation of Mr. Hale, extended discussion took place relative to the future work of the National Research Council in cooperation with the National Academy of Sciences.

Cary T. Hutchinson, *Secretary.*

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# *DYNAMICAL ASPECTS OF PHOTOSYNTHESIS*[1]

By W. J. V. Osterhout and A. R. C. Haas

Laboratory of Plant Physiology, Harvard University

Communicated by G. H. Parker, February 7, 1918

Although a great amount of attention has been paid to photosynthesis, nothing is known of the dynamics of the process. This aspect of the matter especially deserves investigation as furnishing a new point of attack upon this difficult problem.

We cannot analyze the dynamics of photosynthesis without first securing accurate data. A preliminary difficulty lies in the control of temperature. When leaves of land plants are exposed to sunlight, changes of temperature at once take place in the leaf and it is found that even under favorable conditions of control the temperature of the leaf may fluctuate as much as 10°C. in a half hour period.

To avoid this difficulty, the writers have employed certain aquatic plants, consisting of thin layers or filaments, whose temperature can be regulated to a sufficient extent for the purposes of the investigation.

The fronds of the marine alga, *Ulva rigida* (sea lettuce), are so suitable for this purpose that most of the experimental work was confined to them, although other material was used for comparison. These fronds consist of only two layers of cells and are so thin (about 0.078 mm.) that their temperature remains very close to that of the surrounding liquid. A further advantage of thin fronds is that gaseous exchange is extremely rapid.

To obtain data for the study of dynamics, it is necessary to determine at frequent intervals how much photosynthesis has taken place. This was accomplished by a method elsewhere described.[2] This method enables us to measure quickly and accurately the amount of photosynthesis at definite intervals, without subjecting the plants to an abnormally high concentration of $CO_2$ as has heretofore been customary.[3] It depends upon the fact that photosynthesis abstracts carbonic acid from the surrounding solution and renders it more alkaline. By the use of indicators the degree of alkalinity, and consequently the amount of photosynthesis, can be determined with considerable precision.

The amount of photosynthesis is approximately a linear function of the change in PH value in the range here employed.

In order to measure the degree of alkalinity produced by *Ulva* under the influence of sunlight, a piece of the frond was placed in a tube of Pyrex glass filled with sea water and closed as described elsewhere.[2] The tube was then placed in a large water bath (the temperature of which fluctuated less than 1°C.) in direct sunlight. If clouds interfered with the sunlight at any time the experiment was rejected.

Since the plants produce $CO_2$ by respiration this must be taken into consideration. Experiments were conducted under precisely the same conditions, except that light was excluded. They agree in showing that the respiration was practically constant. It cannot, therefore, affect the form of the curve of photosynthesis.

TABLE 1

| PERIOD | NUMBER OF MINUTES REQUIRED TO PRODUCE STANDARD ALKALINITY | TOTAL NUMBER OF MINUTES EXPOSED | AMOUNT OF PHOTOSYNTHESIS | |
|---|---|---|---|---|
| | | | Observed | Calculated |
| 1st | 35.7 | 35.7 | 1 | 0.92 |
| 2nd | 25.9 | 61.6 | 2 | 2.07 |
| 3rd | 23.3 | 84.9 | 3 | 3.18 |
| 4th | 21.7 | 106.6 | 4 | 4.23 |
| 5th | 20.4 | 127.0 | 5 | 5.23 |
| 6th | 20.3 | 147.3 | 6 | 6.22 |
| 7th | 20.5 | 167.8 | 7 | 7.22 |

Average of 5 experiments at 27° ± 0.5°C.

In each experiment the procedure was the same. Freshly collected *Ulva* (young, dark green fronds not more than 3 or 4 inches long) was placed over night in running sea water and covered so that in the morning no light could reach it. In starting an experiment the *Ulva* was placed in a closed tube in sea water containing a trace of alcoholic phenolphthalein and exposed to light until a definite shade of pink was produced.[4] This shade of pink matched that of a standard buffer solution (whose PH value was determined by the gas chain) containing the same concentration of indicator as the sea water (and observed in a Pyrex tube of the same size).

When the standard shade was attained, the time of exposure was noted. The sea water was then poured out of the tube containing *Ulva* (the last drop being removed by shaking), fresh sea water was added, and a new determination was made of the time required to attain the standard shade.

The results obtained are given in table 1 (average of five experiments). The table shows that the time required to reach the standard shade steadily diminishes until a constant value is reached.

This result is surprising, but it has been confirmed by numerous experiments on *Ulva*, as well as by experiments on *Enteromorpha, Spirogyra, Hydrodictyon, Potamogeton*, and other plants.[5]

It is therefore evident that photosynthesis is a process which shows a gradual acceleration until a steady rate is attained.[6] A question of great interest now presents itself: What is the cause of the initial acceleration and why is a steady rate attained after a certain length of time?

The suggestion which first offers itself is that photosynthesis belongs to the class of autocatalytic processes, in which the reaction is catalyzed by one of its own products. Such reactions begin slowly but as more of the catalyzing substance is produced the reaction goes on at an increasinlgy rapid rate until it begins to slow down as the reacting substances are used up. If these substances are constantly renewed, the reaction will not slow down but continue to go on more and more rapidly.

In our experiments on photosynthesis the reacting substances are constantly renewed.[7] The substances entering into the reaction are presumably carbon dioxide and water. The concentration of the water remains constant, while as soon as the concentration of the carbon dioxide has diminished by a very small amount it is brought back to the original point by the renewal of the sea water.

If photosynthesis were an autocatalytic reaction, it should, under these conditions, continue to increase in speed as time goes on. As a matter of fact it soon attains a steady rate. This might be accounted for by supposing that the concentration of the catalyst cannot exceed a certain amount, being limited by its own solubility. But in that case the rate would increase more and more rapidly up to a certain point and suddenly become stationary when the limit of solubility was reached.[8] This is not the case. The rate increases rapidly at first then more and more slowly until it finally becomes stationary.

It might be supposed that the speed of the reaction is checked by the accumulation of the products of the reaction. In that case, however, the rate would not become constant but would gradually diminish to zero. Such influence of the products would be possible only in the case of a reversible reaction and we have no ground for believing that photosynthesis comes under this head.[9]

It might also be suggested that the rate becomes constant through the operation of a 'limiting factor' such as lack of light, carbon dioxide, or of temperature. But it is evident that the effect of such a factor would be fully felt at the very start of the reaction and that it could not cause a gradual falling off in the increase of speed.

This puts clearly before us a fundamental difficulty. The fact that the rate increases most rapidly at first and then more slowly shows that photosynthesis is not an autocatalytic reaction in the usual sense of the word, for in such a reaction[10] the rate would increase slowly at first, then more and more rapidly as time goes on. We must therefore conclude that photosynthesis belongs in a different category.

The key to the situation is furnished by the figures in the second column of table 1 which show that if the reaction is catalyzed by a substance, it must be produced more rapidly at first and then more and more slowly. It is also evident that this substance must be limited in amount and that when its production ceases the rate of photosynthesis stops increasing and becomes constant. We may assume that the rate of photosynthesis is proportional to the amount of the catalyst, which we will call $C$. The figures suggest that this substance may be produced in the manner characteristic of a monomolecular reaction. We may therefore assume that $C$ is produced by a substance $A$, under the influence of sunlight, according to the monomolecular reaction: $A \rightarrow C$.

We may now proceed to test this assumption by calculating the amount of photosynthesis which is to be expected after the lapse of a given time.

According to the ordinary equation for a monomolecular reaction,

$$C = A - Ae^{-KT}$$

in which $T$ is time, $e$ is the basis of natural logarithms, and $K$ is the velocity constant of the reaction.

If the rate of photosynthesis is directly proportional to the amount of $C$, we may, for convenience, put

$$\text{Rate of photosynthesis} = \frac{dP}{dT} = C;$$

hence

$$\frac{dP}{dT} = A - Ae^{-KT}.$$

On integration this becomes

$$\frac{P}{A} = T - \frac{1}{K} + \frac{1}{K}e^{-KT}.$$

When the rate has become constant we find that a unit amount of photosynthesis is produced in 20.4 minutes (average of the last 3 periods in table 1), hence the rate of photosyntheses at that time is $1 \div 20.4 = 0.049$. This is by assumption equal to $C$ when $A$ is completely transformed into $C$ and this is in turn equal to $A$ at the beginning of the reaction. Hence $A$ at the start $= 0.049$. We may substitute this value in the equation and find the value of $K$ by trial. If we put $K = 0.049$ we get the values given in table 1. Better agreement with the observed values is obtained by taking lower values of $K$. This produces a gradual falling off in subsequent values, but it is possible that this might actually occur if the experiment could be continued for a sufficient length of time.

The agreement between the observed and the calculated values is very satisfactory except at the start. In this connection it may be pointed out that at the beginning of a reaction disturbances are to be expected.

It is therefore evident that the assumption justifies itself by giving an adequate quantitative explanation of the observed results. The question then arises whether it is a natural one. It would seem very probable that the light produces a substance which accelerates the reaction and unless this substance is produced in unlimited amount there must come a time when the rate will become steady (or fall off). The assumption therefore seems to be reasonable.

It is attractive to form a hypothesis as to the nature of the catalyst. One might be tempted to suppose that it is chlorophyll but for the fact that some plants which are deep green may not photosynthesize as rapidly as those which possess less chlorophyll.[11] It is of course possible that the less active plants are deficient in some essential factor other than chlorophyll. On the other hand it may be necessary for chlorophyll to be transformed by the light from an inactive into an active form,[12] so that the rate of photosynthesis depends on the amount of 'active chlorophyll' present. This would be analogous to the well known activation of enzymes by various means.

An equally satisfactory quantitative explanation is obtained if we suppose the amount of photosynthesis to correspond to the amount of a substance $P$, produced (under the influence of light) by the reaction

$$S \rightarrow M \rightarrow P,$$

in which $S$ represents a constant source, (i.e., a substance which does not appreciably diminish during the experiment).

We may suppose that in the morning, before the frond is exposed to the light, $S$ alone is present. On exposure to light the formation of $M$ and $P$ occurs. The amount of $M$ will then increase until it reaches a constant value (when its rate of formation is equal to its rate of decomposition) but the value of $P$ will continually increase, since it does not undergo decomposition. When $M$ has reached a constant value we find (putting $K$ as the velocity constant of the reaction $M \rightarrow P$) that the amount of $M$ decomposed in 1 minute (unit time) is $KM$; this is also the amount of $P$ which is formed in 1 minute and since the reaction $S \rightarrow M$ produces just enough of $M$ to balance the loss of $M$ (by transformation into $P$) the amount of $M$ produced each minute is $KM$. Hence if we start in the morning with $S$ alone there will be produced each minute $KM$ and all of this will be transformed into $P$ except what is present at any moment as $M$. Hence the amount of $P$ produced in the time $T$ is $KMT - M$.

We may, for convenience, put $M = 1$ when it has attained its constant value; the rate of increase of $P$ is then constant and we find that it takes 20.4 minutes to produce 1 unit of photosynthesis. Hence $KMT = 1$. Substituting in this equation the values of $M$ and $T$ we have 20.4 $K = 1$ whence $K = 0.049$.

At the start of the reaction the value of $M$ is 0: this gradually increases to 1 and remains constant. During this period of increase the value of $M$ may be calculated as follows: When $M$ has reached its constant value ($M = 1$) let us suppose that the reaction $S \rightarrow M$ suddenly stops while $M \rightarrow P$ contimes; we shall find that if $T$ minutes have elapsed after this occurrence, the amount of $M$ which has disappeared is $1 - e^{-KT}$. If the reaction $S \rightarrow M$ had not stopped it would have produced enough of $M$ so that (in spite of the fact that $M$ is constantly decomposing) the amount of $M$ remaining at the time $T$ would be just enough to balance the loss, or $1 - e^{-KT}$. Hence if we start with nothing but $S$ (the values of $M$ and of $P$ being zero) the amount of $M$ present after the lapse of any given time $T$ will be $1 - e^{-KT}$ and the amount of P will be

$$P = KT - (1 - e^{-KT})$$

This becomes the same as the equation

$$\frac{P}{A} = T - \frac{1}{K} + \frac{1}{K} e^{-KT},$$

when in the latter we put $K = A$ as was done in making the calculations given in table 1. Hence when we substitute the value $K = 0.049$ in the equation $P = KT - (1 - e^{-KT})$ we obtain the values already given in table 1.

If the chlorophyll takes part in the reaction by decomposing or by combining (as some recent evidence indicates), we might suppose that $S$ represents inactive chlorophyll, $M$ active chlorophyll and $P$ a derived substance which combines with $CO_2$. At present it does not seem profitable to attempt a more extended discussion of this question. But it may be pointed out that (as one of us has recently emphasized)[13] consecutive reactions of the type here discussed, are to be looked upon as the rule, rather than as the exception, in living matter.

It is evident that either of the theories developed above gives a quantitative explanation of the results. Both seem to be based on reasonable assumptions. Future investigation must decide which is more useful.

In any event, it is clear that much is to be learned concerning the dynamics of photosynthesis, and it is hoped that the considerations here set forth may be of value in this connection.

*Summary.*—*Ulva* which has been kept in the dark begins photosynthesis as soon as it is exposed to sunlight. The rate of photosynthesis steadily increases until a constant speed is attained.

This may be explained by assuming that sunlight decomposes a substance whose products catalyze photosynthesis or enter directly into the reaction.

Quantitative theories are developed to account for the facts.

[1] Preliminary communication.

[2] The paper will appear shortly in *Science, New York,* N. S., 1918. All the precautions mentioned in this account were carefully observed in the present investigation.

[3] The methods previously used in studying the photosynthesis of aquatic plants are not as accurate as the one here described, nor do previous experiments afford the kind of data needed for our purpose. Cf. Blackman, F. F., and Smith, A. M., *Proc. Roy. Soc., London,* (B) **83**, 1911, (389).

[4] All matching of shades was done under a 'Daylight' lamp, so that uniform conditions were assured throughout the experiments. Cf. *Science, New York,* N. S., **42**, 1915, (764). A clear space was left in the tube below the *Ulva* to facilitate comparison of colors. In any single experiment the buffer may be dispensed with by using as a standard the pink solution produced by the first exposure. The first exposure should be as short as is consistent with obtaining a definite standard. Experiments showed that the trace of alcoholic phenolphthalein had no injurious effect.

[5] In experiments on fresh water algae a small amount of sodium bicarbonate was added to the water.

[6] This acceleration is not due to the increase in the intensity of light as the sun gets higher for it was also observed when the experiments were started at noon.

[7] When the sea water is not changed during the experiment the curve rises more rapidly at first then bends over to the right as the supply of $CO_2$ is used up.

[8] This is because the catalyst from the moment of its production is in solution. It is not analogous to a solid going into solution, which dissolves more slowly as the limit of solubility is approached.

[9] While respiration is in a sense the opposite of photosynthesis the steps in the process are apparently quite different from those found in photosynthesis.

[10] I. e., under the conditions of the present experiment, where the reacting substances are kept approximately constant in composition.

[11] Aquatic plants taken directly from ice-covered ponds in winter are found to possess but feeble photosynthetic power, though of a deep green color.

[12] The activation of substances by light is well known in photochemistry.

[13] *J. Biol. Chem., New York,* **21**, 1917, (585); **22**, 1917, (23).

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# *MOBILITIES OF IONS IN AIR, HYDROGEN, AND NITROGEN*

By Kia-Lok Yen

Ryerson Physical Laboratory, University of Chicago

Communicated by R. A. Millikan, January 21, 1918

In spite of the great number of investigations devoted to gases the question whether an ion is a molecule or an atom carrying an elementary charge, or whether it is a number of neutral molecules clustering about a charge is not as yet definitely settled. Both the 'cluster' hypothesis, according to which an ion is conceived of as a unit elementary charge surrounded by a satellite of neutral molecules, and the 'small-ion' hypothesis, according to which an ion is conceived of as a single molecule carrying an elementary charge, explain equally well the phenomenon which first necessitated the former, and also the older, hypothesis; this phenomenon being the fact that the mobilities and the diffusion coefficients of the ions in gases are relatively small in comparison with those of the uncharged molecules in the same gases. They also explain equally well a certain number of other phenomena, but experiments designed to test directly the validity of one or the other of these hypotheses led to contradictory results.

The measurement of the mobilities of ions under various pressures and different electric field-strengths has always been generally conceded as the mode of attack in the solution of the problem. For if the older, the cluster, hypothesis is true the cluster would break up when the ion acquired a sufficiently high kinetic energy and the mobility would increase abnormally. But if the small-ion theory is true there would be no such dissociation and the mobility would remain normal. Quite a number of such measurements have been made, and they gave contradictory results. For instance, Latty,[1] Kovarick,[2] Todd,[3] Townsend,[4] Franck,[5] Moore,[6] Haines,[7] and Ratner[8] found the mobility to increase abnormally, whereas Chattock,[9] Wellisch,[10,11] and Loeb[12] found it normal over a wide range of potentials applied. In view of these results further experiments seemed desirable and hence the work herein described was undertaken.

The method here employed was the Rutherford[13]-Franck[14] method as employed by Loeb.[12] The apparatus was fully described in Loeb's articles.

*Results: a. Air.*—An attempt first to repeat Loeb's work with air gave the following results:

TABLE 1

TABLE OF RESULTS OBTAINED FOR IONIC MOBILITIES IN AIR, FEBRUARY–MARCH, 1917

| $U+$ | $U-$ | $X+$ | $X-$ | $P$ | $K+$ | $K-$ | $R$ |
|---|---|---|---|---|---|---|---|
| *1. 60 cycles, 119 volts* | | | | | | | |
| 1.12 | 1.75 | 168 | 134 | 746 | 1.10 | 1.72 | 1.56 |
| 1.12 | 1.75 | 168 | 134 | 746 | 1.10 | 1.72 | 1.56 |
| 1.12 | 1.75 | 168 | 134 | 752 | 1.10 | 1.73 | 1.56 |
| 1.25 | 1.73 | 168 | 134 | 742 | 1.22 | 1.70 | 1.38 |
| Mean | | | | | 1.14 | 1.72 | 1.51 |
| *2. 14,758 cycles, 5000 volts* | | | | | | | |
| 1.64 | 1.98 | 14,160 | 12,870 | 752 | 1.62 | 1.92 | 1.21 |
| 1.57 | 1.84 | 14,160 | 12,870 | 750 | 1.56 | 1.82 | 1.17 |
| 1.64 | 1.98 | 14,160 | 12,870 | 749 | 1.61 | 1.95 | 1.21 |
| 1.57 | 1.84 | 14,160 | 12,870 | 746 | 1.55 | 1.81 | 1.17 |
| 1.82 | 2.10 | 13,810 | 12,650 | 692 | 1.66 | 1.92 | 1.17 |
| 2.26 | 2.61 | 12,300 | 11,550 | 558 | 1.66 | 1.91 | 1.16 |
| Mean | | | | | 1.61 | 1.90 | 1.18 |
| Mean of both sets | | | | | 1.37 | 1.81 | 1.34 |

$U+$ = Mobility of positive ions. $U-$ = Mobility of negative ions. $X$ = Field strength in volt/cm. $P$ = Pressure in mm. $R = K-/K$. $K$ = Mobility at 760 mm. pres. $X-/P$ Max. = 20.70, Min. = 0.18. $X+/P$ Max. = 22.04, Min. = 0.22.

It may be seen from the above table that the mean values of $R$, the ratio of the negative to the positive mobilities, are different for the two frequencies employed, and it appears as though the mobilities did vary—since the ratio could not vary unless either or both of the mobilities did. But the following considerations will show that this difference is ascribable to experimental variations. In the first place, the results of different experimenters, and even those of the same experimenter, show a maximum variation of almost 30%, for instance, from $R = 1.16$ to 1.37 (Wellisch), or from $R = 1.15$ to 1.93 (Loeb). Both of these authors attribute the variations to external variations of their experiments. Thus a slight external variation of some kind is liable to cause such a variation in the ratio without the mobilities themselves being varied at all. Furthermore, if the mobilities tend at all to vary with field-strength, their variations would be much greater than are manifested since the field-strength is 168 volt/cm. in one case and 14,160 volt/cm. in the other. Or, it may be said that even if the ratio does vary about 20% when the field-strength is increased from 169 to 14,160 volt/cm. (about 8333%), it may be considered constant for all practical purposes. However, there were enough of the experimental uncertainties to account for the variation.

TABLE 2

RESULTS OBTAINED FROM THE MEASUREMENTS ON HYDROGEN, MAY–JUNE, 1917

| $U+$ | $U-$ | $X+$ | $X-$ | $P$ | $K+$ | $K-$ | $R$ |
|---|---|---|---|---|---|---|---|
| *14,758 cycles, 4000 volts* | | | | | | | |
| 5.51 | 8.20 | 6669 | 5668 | 748 | 5.43 | 8.10 | 1.49 |
| 5.92 | 8.20 | 6669 | 5668 | 748 | 5.81 | 8.10 | 1.38 |
| 5.51 | 8.20 | 6669 | 5668 | 746 | 5.40 | 8.10 | 1.49 |
| 8.20 | 12.21 | 5668 | 4723 | 518 | 5.58 | 8.35 | 1.49 |
| 14.94 | 20.99 | 4192 | 3524 | 290 | 5.70 | 8.15 | 1.41 |
| 14.94 | 20.99 | 4192 | 3524 | 300 | 5.84 | 8.35 | 1.41 |
| Mean | | | | | 5.56 | 8.19 | 1.45 |

Thus the conclusion is that between the field-strength of 168 and 14,160 volt/cm. the mobility of the positive ions remains absolutely normal, and the mobility of the negative ions remains normal also between 134 and 12,870 volt/cm.

These results more than amply substantiate those obtained by Loeb and it is therefore quite safe to conclude that the evidences obtained so far point decidedly in the direction of the small-ion theory.

*b. Hydrogen.* The results of the measurements in hydrogen with the high frequency high potential field are shown in table 2.

It may be seen from the table that with a potential gradient of 6669 volt/cm. or $X/p = 14.45$, the positive mobility remains normal. The negative mobility also remains absolutely normal with 5668 volt/cm. or $X/p$ = 12.15. Thus it may be concluded that the law $Up$ = constant, where $U$=mobility and $p$ = pressure in mm. mercury, is verified for hydrogen up to these limits.

Besides the normal positive and negative ions the existence of free negative electrons in hydrogen was proved. These electrons existed in abundance when the gas was freshly prepared and disappeared entirely after the gas had remained in the ionization chamber for about six or eight hours. The disappearance of the electrons might conceivably be the result either of their fast dissipation into the walls of the chamber or of their ready formation of negative ions with the neutral molecules of either hydrogen or the impurities from the sealing wax that had evaporated into the chamber in the meantime.

TABLE 3

| $U+$ | $U-$ | $X+$ | $X-$ | $P$ | $K+$ | $K-$ | $R$ |
|---|---|---|---|---|---|---|---|
| *60 cycles* | | | | | | | |
| 5.28 | | 77.8 | | 748 | 5.21 | | |
| 5.50 | 8.80 | 25.0 | 16.5 | 746 | 5.41 | 8.65 | 1.52 |
| 6.60 | 11.19 | 20.0 | 13.0 | 600 | 5.22 | 8.80 | 1.62 |
| 8.95 | 13.28 | 29.5 | 24.0 | 498 | 5.85 | 8.70 | 1.48 |
| 13.90 | 22.00 | 19.8 | 11.5 | 300 | 5.49 | 8.69 | 1.58 |
| 22.91 | 31.06 | 16.5 | 99.0 | 198 | 5.95 | 8.15 | 1.34 |
| | 9.26 | | 28.0 | 746 | | 9.26 | |
| Mean........................................ | | | | | 5.52 | 8.71 | 1.57 |

$X-/P$ Max. = 12.15, Min. = 0.38. $X+/P$ Max. = 14.45, Min. = 0.66.

An effort was made to search for the two other kinds of negative ions which Haines claimed to have found.[7] And as no trace of these other ions could be found it was thought that the disposition of the apparatus employed might not have been sufficiently adequate for their detection. Consequently it was considered desirable to repeat Haines' experiment in order to rectify the present method. Thus Haines' experimental conditions were reproduced as exactly as possible according to his descriptions with the expectation of obtaining similar results.

The results of this operation are shown in table 3.

These results agree with those obtained in the employment of the high frequency high potential field in showing that no such intermediate negative ions existed. This, together with a careful study of Haines' curves, led to the conclusion that in so far as experimental results are concerned there is not a scrap of evidence, either in Haines' results or in those obtained in the present experiment, of these other species of negative ions which were claimed to exist by Haines.

It may be proper to mention here that no sooner had the above conclusion been arrived at than it received corroboration from Wellisch's latest paper[18] in which it was reported that no trace could be found of Haines' ions *B* and *C*.

Incidentally an interesting fact was revealed in the comparison between the two sets of curves. If was found that the amount of free negative electrons present in the gas was smaller when under high than when under low potential. This would seem to suggest that the electrons—or some of them at least—did actually attach themselves to neutral molecules when a high potential was applied and thus formed negative ions. This would not be at all impossible since the tremendous velocity imparted to them by the high field would enable them to produce ions from neutral molecules by attaching themselves to the latter. It would be interesting to find out where, that is, at what potential—other conditions remaining the same—this sort of ionization actually would begin.

TABLE 4

Results Obtained for Nitrogen, July, 1917

| $U+$ | $U-$ | $X+$ | $X-$ | $P$ | $K+$ | $K-$ | $R$ |
|---|---|---|---|---|---|---|---|
| *1. 60 cycles* | | | | | | | |
| 17.60 | 22.96 | 15.0 | 11.5 | 60 | 1.39 | 1.81 | 1.30 |
| | 10.15 | | 26.0 | 140 | | 1.87 | |
| 2.81 | 3.88 | 47.0 | 33.5 | 360 | 1.33 | 1.84 | 1.38 |
| 1.27 | 1.65 | 51.0 | 40.0 | 750 | 1.26 | 1.63 | 1.30 |
| 1.36 | 1.84 | 50.0 | 38.0 | 745 | 1.33 | 1.80 | 1.35 |
| 1.34 | 1.82 | 49.8 | 38.0 | 745 | 1.31 | 1.78 | 1.36 |
| Mean........................................ | | | | | 1.32 | 1.79 | 1.34 |
| *2. 14,758 cycles, 5000 volts* | | | | | | | |
| 1.31 | 1.84 | 17,670 | 14,880 | 750 | 1.29 | 1.82 | 1.40 |
| 1.31 | 1.84 | 17,670 | 14,880 | 745 | 1.28 | 1.80 | 1.40 |
| 1.31 | 1.84 | 17,670 | 14,880 | 742 | 1.28 | 1.80 | 1.40 |
| 2.76 | 3.93 | 13,910 | 10,110 | 360 | 1.31 | 1.86 | 1.42 |
| 2.76 | 3.93 | 13,910 | 10,110 | 345 | 1.26 | 1.78 | 1.42 |
| Mean........................................ | | | | | 1.28 | 1.81 | 1.41 |
| Mean of both frequencies ........................... | | | | | 1.30 | 1.80 | 1.38 |

$X-/P$ Max. = 29.0, Min. = 0.05. $X+/P$ Max. = 40.0, Min. = 0.07.

*c. Nitrogen.*—Table 4 shows the results of the mobility measurements in nitrogen. The maximum potential gradient employed was 17,670 volt/cm. for the positive and 14,880 volt/cm. for the negative ions. The mobilities remained absolutely normal up to these limits and the law $Up$ = constant was found to be applicable here as it was in the case of air and hydrogen.

Here too an abundance of free negative electrons was found, although the amount was not so great as that found in hydrogen under the same pressure.

*Conclusion and Discussion.*—As the results of the present experiment exhibit no deviation from the law $Up$ = constant it follows that both the positive and the negative ions did not disintegrate at the potentials employed. It may be seen from the tables that the values of $X/p$ were very close to the values at which sparking would occur in the respective gases. And since the cluster hypothesis demands the disintegration of the ions when the values of $X/p$ are much lower than those employed,[15] it therefore follows that these results are directly contradictory to this hypothesis.

On the other hand, the results are in perfect accord with the small-ion hypothesis. Taking this in conjunction with the results of other experiments, especially those of Wellisch and Loeb, there does not seem any question at all regarding the validity of this hypothesis.

There remains, however, an experimental fact which the cluster hypothesis seems to be able to explain better than the small-ion hypothesis, and that is the difference between the mobilities of the positive and the negative ions. For, if both the positive and the negative ions are single molecules carrying elementary charges different only in signs, why should they have different mobilities? Whereas if the ions are clusters the difference in their mobilities may be ascribed to the difference between the numbers of molecules constituting the two kinds of ions.

This difficulty of the small-ion theory, however, is more apparent than real in view of the recent theories as to the electronic constitution of matter. If an atom consists of a positive nucleus surrounded by a satellite of negative electrons held together by the attractive force from the nucleus, the phenomenon of ordinary molecular collision must be attributed to the repulsion between the two systems of negative electrons in the colliding molecules[16, 17]. Since, according to the small-ion theory, the only difference between the uncharged molecules and the ions lies in the number of negative electrons in the satellites—the negative ion having one more electron, and the positive ion one less than the uncharged molecules—it follows that the only difference between the ordinary molecular collisions and the collisions between ions and uncharged molecules is that between the numbers of electrons in the colliding systems. It is only reasonable, therefore, to extend the conception of the ordinary molecular collision to cover the case of collision between ions and uncharged molecules. Now since the negative ion has more of these peripheral negative electrons than the positive ion it follows that the repulsion between the negative ion and the uncharged molecule is greater than that between the positive ion and the uncharged molecule; and the attractive force between the negative ion and the uncharged molecule is smaller than that between the positive ion and the uncharged molecule. This results in a difference in the *effective mean free paths* of the two kinds of ions. The positive ion, by virtue of the greater attractive force existing between it and the uncharged molecules, drags the latter more towards it and thus has a smaller *effective mean free path.* The negative ion, with the smaller attractive force, has a greater *effective mean free path.* As the mobility varies directly with the mean free path, it can be easily seen why the negative ions have a greater mobility than the positive ions.

But the above explanation would not be applicable to the case where the ratio of the negative to the positive mobility is less than unity, for that would mean that the attractive force between the negative and the uncharged molecule is greater than that between the positive ion and the uncharged molecule, which would be impossible according to the theory upon which the explanation is based. Fortunately, in such cases the differences between the positive and the negative mobilities are so small that they are well within the limits of experimental fluctuations; consequently, until it is conclusively established that there are cases where the positive mobilities are greater than the negative by a quantity much too great to be accounted for by experimental conditions, the above explanation seems to be the most reasonable one so far advanced.

The detailed paper has been communicated to the *Physical Review.*

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# THERMO-ELECTRIC ACTION WITH DUAL CONDUCTION OF ELECTRICITY

By Edwin H. Hall

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Communicated February 16, 1918

In a paper[1] presented before the National Academy of Sciences in November, 1917, I discussed thermo-electric action in metals on the hypothesis of progressive motion by the 'free' electrons only. I have now extended the discussion to the case of dual electric conduction; that is, conduction maintained in part by the free electrons (electrons F) and in part by the associated electrons (electrons A), the latter passing directly from atomic union to atomic union.

I do this because, though we may not at present have a satisfactory theory[2] of electric conduction involving such action of the associated electrons, we are equally far from having a satisfactory theory of conduction as a function of the free electrons only.

I take it as self-evident that, whereas electric potential-gradient acts upon both free and associated electrons, tending to carry them in the direction of decreasing negative potential, free-electron pressure-gradient acts on the free electrons only. The necessary result of this consideration is the conclusion that, if electrons (A) as well as electrons (F) can move progressively through a metal, we shall, in a detached bar of metal having a temperature gradient from one end to the other, have a constant procession of free electrons from the place of high electron-gas pressure, the hot end of the bar, toward the place of low pressure, the cold end, while an equal procession of associated electrons moves in the opposite direction. For the mechanical tendency of the free electrons toward the cold end maintains an excess of negative potential at this end, with a corresponding deficiency at the hot end; and the electric potential-gradient thus established drives associated electrons from cold to hot, while it opposes, without being able entirely to prevent, the movement of free electrons from hot to cold.

The state of mobile electrical equilibrium thus presented to our imagination involves no violation of commonly accepted principles. The slight, extremely slight, reduction of electron mechanical pressure below the normal equilibrium pressure at the hot end of the bar induces there continual passage of the electrons from the associated to the free state, evaporation, let us say, with absorption of heat. At the cold end, on the other hand, the very slight excess of electrical mechanical pressure, above the normal value proper to the temperature, induces continual passage of electrons from the free to the associated state, condensation, let us say, with release of heat. The whole operation carries heat from the hot to the cold end of the bar, and it is, in fact, somewhat analogous to the action of a steam heating-system, the free-electron movement corresponding to the stream of steam and the associated electrons movement to the return flow of the water.

My conception of the action, for a case in which there is no lateral loss or gain of heat, is set forth diagrammatically in figure 1, in which the dotted lines indicate movement of the free electrons and the full lines movement of the associated electrons.[3]

In my previous paper,[1] dealing with the hypothesis of progressive motion of 'free' electrons only, I rejected as unnecessary the assumption of a specific attraction of metals for electrons. But with the more complicated conditions dealt with in the present paper we cannot avoid this assumption; for without it we should have thermal conduction without initial difference of temperature, and so a violation of the second law of thermodynamics, in a bar of alloy varying in composition from metal $\alpha$ at one end to metal $\beta$ at the other. There must be no progressive movement of free electrons from one end to the other, or of associated electrons in the opposite direction, in such a bar. The superior mechanical pressure of the free electrons at the $\alpha$ end will produce a slight initial movement establishing a potential gradient along the bar, the $\alpha$ end becoming positive and the $\beta$ end negative; but the influence of this potential gradient on the associated electrons must be offset by a specific atomic attraction directed toward the $\beta$ end. It is reasonable to suppose that the smaller free-electron density at the $\beta$ end is due to the superior attraction of the $\beta$ positive nucleus compared with the $\alpha$ nucleus. The resulting equilibrium, the inhibition of circulation of the electrons from one end to the other of the bar and back, is analogous to the equilibrium in a system made up of water, water-vapor, and a solution-column sustained by osmotic pressure. The upper end of such a column, with its reduced vapor-pressure, corresponds to the $\beta$ end of our bar with its small free-electron pressure. The force of gravitation, directed from the top to the bottom of the column, is analogous to the electrical potential gradient from the $\beta$ end toward the $\alpha$ end of the bar.

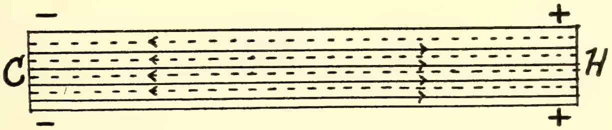

FIG. 1

Naturally the question arises whether a bar of one metal having a temperature gradient, and a free electron density rising with the temperature, has not a differential specific attraction tending to move electrons toward the cold end. There may be such an attraction, and it will be well for us to take account of it, but where there is a temperature-gradient we are not obliged to suppose a dead-lock between the potential gradient and this differential specific attraction. The conception of electron circulation, with thermal conduction (or convection), in a detached unequally heated bar survives the admission of specific attraction; but the whole matter now becomes more involved.

In addition to the potential, $P$, due to electric charge, we must now think of a potential, $P_a$, due to the differential attraction of the unequally heated, unhomogeneous, metal for the associated electrons, and also of a potential, $P_f$, due to the differential attraction of the metal for the free electrons. Both classes of molecules are subject to the charge-potential $P$, but electrons (A) only are subject to the potential $P_a$, and electrons (F) only are subject to the potential $P_f$.

*Under hypothesis* (A): If we assume, as hypothesis (A), that the mechanical tendency of the free electrons, if acting without electric forces, would produce equality of pressure from end to end of the bar, the condition of equilibrium (see fig. 1) in a detached bar hot at one and cold at the other is

$$\mu\left[\left(\frac{dl}{dl}\,.\,dl + \frac{d\,(P+P_b)}{dl}\,.\,nedl\right) \div nmdl\right] ne = -\,k_a\frac{d\,(P+P_a)}{dl} \tag{1}$$

where $(dp/dl)$ is the gradient of mechanical pressure of the free electrons along the bar of length $l$, $n$ is the number-density of the free electrons in the metal, $m$ is the mass and $e$ the charge of an electron, $\mu$ is the coefficient of mobility of the free electrons through the metal, and $k_a$ is the electric conductivity of the metal, so far as conductivity is due to the electrons (A).

A simple formula,

$$\mu = k_f\,.\,\frac{1}{Gme^2}, \tag{2}$$

where $G$ is $(1/m)$, connects $\mu$ with the free-electron specific conductivity, $k_f$. Evidently

$$G = nv, \tag{3}$$

where $v$ is the volume of one gram of free electrons in the metal.

Very simple operations, using equations (2) and (3), derive from (1) the form

$$(P_c - P_h) + \int_h^c \frac{k_f}{k_a + k_f}\,.\,dP_f + \int_h^c \frac{k_a}{k_a + k_f}\,.\,dP_a = \frac{1}{Ge}\int_c^h \frac{k_f}{k_a + k_f}\,.\,vdp, \tag{4}$$

in which the integration extends from the hot end $(h)$ to the cold end $(c)$ of the bar.

The first member of this equation is the amount of reversible work that would be done by or on the unit quantity of electricity in passing *through* the bar, if it were made part of a closed thermo-electric circuit. This is something different from, probably smaller than, $(P_c - P_h)$, which is the *charge* potential-difference between the two ends of the bar. The quantity expressed by the whole first member I shall call the *virtual* e.m.f., resident in the bar because of its temperature gradient.

The form of the second member shows that the virtual e.m.f. can be represented by an area on the $P$-$V$ plane. Thus, if the line $A\ D$ in figure 2 represents the pressure-volume relations of the free electrons for the whole length of the bar, so that

$$\text{area } EADG = \int_c^h vdp,$$

we shall have

$$\text{area } EA'D'G = \int_c^h \frac{k_f}{k_a + k_f} \, . \, vdp,$$

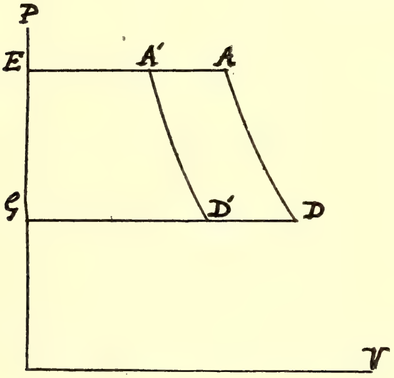

FIG. 2

provided we make the width of this area correspond at every height to the value of $k_f/(k_a + k_f)$ for that height.

Without the conception of dual conductivity and specific attraction we should, as my previous paper[1] shows, have in place of (4) the simple equation

$$P_c - P_h = \frac{1}{Ge} \int_c^h vdp.$$

with dual conductivity but without specific attraction we should have

$$P_c - P_h = \frac{1}{Ge} \int_c^h \frac{k_f}{k_a + k_f} \, . \, vdp.$$

Obviously, then, the participation of the electrons (A) in the conductivity reduces the e.m.f. due to the temperature gradient in the bar. In fact, the part which associated electrons play in thermo-electric action is analogous to that played by entrained water in the work done by steam. The larger the proportion of water, the smaller is the mechanical effect per unit mass of the mixture.

In the isothermal alloy 'bridge,' of composition varying from pure $\alpha$ at one end to pure $\beta$ at the other end, which is supposed to connect the two metals at their hot or at their cold ends, we must have, when it stands detached, no cyclic movement of the electrons. Accordingly we get, in place of equation (1), the two equations

$$dP + dP_a = 0 \tag{5}$$

and

$$dP + dP_f = -\frac{dp}{ne} = -\frac{1}{Ge} . vdp. \tag{6}$$

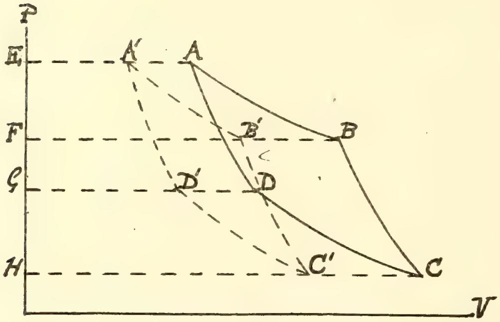

FIG. 3

These lead to the following, as the expression for the *virtual* e.m.f. due to the non-homogeneity of the isothermal bridge:

$$\int_\alpha^\beta \frac{k_f}{k_a + k_f}(dP + dP_f) = \frac{1}{Ge}\int_\beta^\alpha \frac{k_f}{k_a + k_f} . vdp. \tag{7}$$

The first member of (7) is the reversible work that would be done, on the free electrons only, during the passage of the unit quantity of electricity through the bridge.

The expression for the work done on the electrons (A) is absent here, for the reason that, according to equation (5), no *reversible* work would be done on them.

For a thermo-electric circuit, made up of a bar of metal $\alpha$, a bar of metal $\beta$, and two isothermal alloy bridges $\alpha$–$\beta$, we find the net, or total, virtual e.m.f. to be represented by $(1 \div Ge)$ times the area $A'\ B'\ C'\ D'$ in figure 3, where $A\ B$, $B\ C$, $A\ D$, and $D\ C$ indicate the pressure-volume relations of one gram of free electrons in the four parts of the circuit respectively, and $A'\ B'$, $B'\ C'$, $A'\ D'$, $D'\ C'$, are found respectively from the corresponding full lines by means of the ratio $k_f \div (k_a + k_f)$, applied as in figure 2.

In spite of the conspicuous part which the specific potentials $P_a$ and $P_f$

play in the local virtual e.m.fs. of the circuit, we can, by making a gap in any isothermal homogeneous part of this circuit and inserting there an electrometer, measure the total e.m.f. as a simple difference of charge-potential. The total, or net, amount of work done by or against the specific attractions which enter into $P_a$ and $P_f$ is zero for any quantity of electricity which goes completely through the circuit.

*Under hypothesis* (B): If, in place of hypothesis (A), we assume that the mechanical tendency of the free electrons is towards the condition of equilibrium which holds for thermal effusion, each local virtual e.m.f. will be represented by an area like $E'\ A'\ D'\ G'\ g'e'$ in figure 4, where $E'$ is the mid-point of $E\ A'$ and $G'$ is the mid-point of $G\ D'$. But the combination of four such areas, one for each of the lines $A'\ B'$, $B'\ C'$, $A'\ D'$, and $D'\ C'$, of figure 3, will give precisely the same net result that is represented in figure 3 by $A'\ B'\ C'\ D'$. The total e.m.f. is, then, precisely the same under hypothesis (B) as under hypothesis (A). This is because the fundamental conditions of $p$ and $v$, represented by the lines $A\ B$, $B\ C$, $C\ D$, and $D\ A$, in figure 3, remain substantially the same under hypothesis (B) as under hypothesis (A).

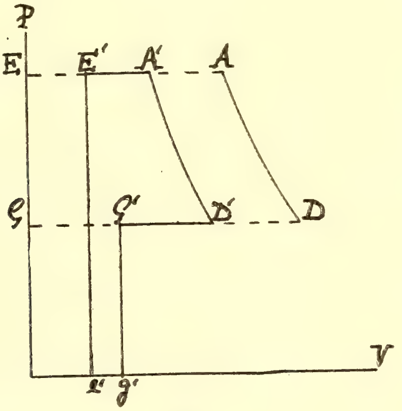

FIG. 4

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[1] These Proceedings, 4, 1918, (29–35).

[2] For suggestions see a paper by myself in these Proceedings, March, 1917, and one by P. W. Bridgman in the *Physical Review*, April, 1917, p. 269.

[3] I am not without hope that the mechanism here suggested will prove to be of great service in the theory of heat conduction. It seems probable that the free electrons within a metal are quite incapable, acting as a permanent gas, of accounting for the magnitude of the metal's heat conductivity. But it is a familiar fact that the heat-carrying power of a vapor, involving evaporation and condensation, is vastly greater than that of a permanent gas. It appears, from the imperfect data now at my command, that the operation illustrated by figure 1 would give thermal conductivity of the right order of magnitude.

This conception of thermal conductivity, a conception occurring quite incidentally and unexpectedly, has already been communicated to the American Physical Society in a paper read at the meeting of December, 1917.

# TERRESTRIAL TEMPERATURE AND ATMOSPHERIC ABSORPTION

By C. G. Abbot

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Read before the Academy, November 21, 1917

The earth's temperature depends mainly on the balance of incoming solar energy and outgoing terrestrial energy of radiation. These two classes of rays lie chiefly in two far-separated regions of spectrum. Of solar rays, 98% lie between 0.3 and 3.0 microns ($\mu$) of wave-length. Of terrestrial rays about the same proportion lie between 5 and 50 microns. According to Abbot and Fowle's researches, about 40% of the solar rays directed towards the earth are reflected to space. The earth must radiate to space $1.93 \times 0.60 \times 0.25$, or 0.29 calorie per cm$^2$ per minute on the average from its whole surface to keep a steady temperature in balance with the solar rays received over the area of its cross section. If the earth's surface was a perfect radiator and its radiation passed unhindered to space, it would emit according to Stefan's law $8 \times 10^{-11} \times (287),^4 = 0.55$ cal. per cm$^2$ per minute. How shall we explain the discrepancy between 0.29 and 0.55 calories?

1. Is the earth's surface a perfect radiator? Its surface is about three-fourths water. Of the remainder much is moist soil or moist vegetation. The radiative power of the earth must therefore be near that of water. My colleague, Mr. Aldrich, has lately studied the absorbing and reflecting powers of water for long-wave rays. He finds that of the rays emitted by lamp-black paint at 100°C. a layer of water 1 cm. thick transmits none and reflects as follows:

| | | | | | | |
|---|---|---|---|---|---|---|
| Incidence | 0° | 30° | 55° | 63° | 70° | 72° |
| Reflection | 2% | 3% | 7% | 10% | 17% | 22% |

As the absorption is 1− (Refl. + Trans.) he computes that of rays reaching a water surface from a hollow hemispherical enclosing lamp-black-painted surface at 100°, the absorption would be 90%. Experiments on lamp-black paint having shown nothing strongly selective about its radiation in this region of spectrum, we seem justified in concluding, in accord with Kirchoff's law, that water is a 90% perfect radiator in this region of spectrum. As is water, so is the earth's surface. Hence we conclude that the earth's surface sends out 0.50 calorie per cm$^2$ per minute on the average.

2. How much of this does the atmosphere transmit? My colleague Mr. Fowle has recently published[1] results of a long investigation of this subject in which he studied the spectrum up to a wave-length of 17$\mu$ by aid of a spectro-bolometer with rock salt prism. He employed a very long tube in which the beam traversed paths of air up to 250 meters in length containing quanti-

ties of water vapor up to the equivalent of 0.3 cm. of precipitable water. He also observed the solar spectrum to $17\mu$ through paths of atmosphere containing water vapor up to the equivalent of 3.0 cm. of precipitable water. Since rock salt ceases to be sufficiently transparent beyond $17\mu$ Fowle's spectrum work stopped there. But Aldrich, on Mount Wilson, by experiments not yet entirely finished, seems to have shown that neither incoming sun-rays nor outgoing earth-rays non-transmissible to rock-salt (that is over $17\mu$ in wave-length) can traverse the atmosphere. Assuming that this result will be confirmed we have the following results from Fowle's and Aldrich's investigations representing the output and atmospheric transmission of rays from a perfect radiator at earth temperature.

*Per cent of atmospheric transmission for stated cm. ppt.* $H_2O$

| WAVE-LENGTH | INTENSITY | CM. 0.003 | CM. 0.03 | CM. 0.3 | CM. 3.0 |
|---|---|---|---|---|---|
| μ μ | | | | | |
| 4– 5 | 50 | 15 | 45 | 70 | 95 |
| 5– 6 | 142 | 16 | 43 | 66 | 95 |
| 6– 7 | 242 | 45 | 85 | 95 | 100 |
| 7– 8 | 315 | 13 | 42 | 85 | 100 |
| 8– 9 | 360 | 0 | 2 | 40 | 50 |
| 9–10 | 380 | 0 | 0 | 0 | 15 |
| 10–11 | 370 | 0 | 2 | 5 | 40 |
| 11–12 | 350 | 0 | 0 | 4 | 10 |
| 12–13 | 320 | 0 | 0 | 13 | 20 |
| 13–16 | 810 | 100 | 100 | 100 | 100 |
| 16–20 | 510 | 90 | 100 | 100 | 100 |
| > 20 | 1,450 | 100 | 100 | 100 | 100 |
| 4–∞ | 5,300 | 49 | 57 | 66 | 75 |

From these results Fowle has computed that in clear weather, when precipitable water in the atmosphere is 1 cm., the atmosphere transmits 28% to space of the radiation emitted by the earth's surface. In the tropics where a load of atmospheric humidity equal to precipitable water of 3 cm. or more is common, the transmission would not exceed 20% on clear days. A. Angström has shown[2] that on cloudy nights practically all radiation from the earth's surface to space is cut off. Hence (as it is cloudy half the time on the average of the earth's surface) out of 0.50 calories per square centimeter per minute emitted, the average escape to space, taking both clear weather and cloudy, is only 0.06 calories. As 0.29 calories per $cm^2$ per minute on the average must leave the planet earth, and as the earth's surface contributes, only 0.06 calories, it follows that the atmosphere is the main radiating source, furnishing three-fourths of the output of radiation of the earth as a planet.

Principal sources of the atmospheric radiation in order of their importance are: (1) The clouds; (2) water-vapor; (3) ozone; (4) carbon-dioxide. There is little difference between the importance of the ozone band at 10 μ and the carbon-dioxide band at 15 μ except that the former falls at a point in the spectrum where terrestrial radiation is most intense, and where water-vapor has almost no absorption, while the latter falls at a place where the radiation is not so intense and where water-vapor also absorbs powerfully.

No ozone band was found by Fowle in his work with the long tube, but in the solar spectrum it shows strongly. This accords with work of others who show that ozone is found only at high atmospheric levels. Apparently there is not enough ozone in the atmosphere to produce complete absorption in its band at 10 μ, and it may be that the earth's temperature would be profoundly altered if the ozone contents of the air could be changed. If it were possible, for instance, to charge the surface air above citrus fruit orchards strongly with ozone on a frosty night, perhaps hurtful frosts could thereby be warded off.

Carbon-dioxide exists in the atmosphere so plentifully that its full possible influence seems probably to be exerted. No increase of $CO_2$ would seem likely to produce a considerable effect on terrestrial temperature, and it is probable that the $CO_2$ content of the air could be reduced to less than a quarter of its present amount without notable temperature effects.

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[1] *Smithsonian Misc. Coll., Washington,* **68,** No. 8.

[2] *Ibid.,* **65,** No. 3, p. 54.

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# *MOBILITIES OF IONS IN VAPORS*

By Kia-Lok Yen

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Communicated by R. A. Millikan, January 21, 1918

In a former paper on the Mobilities of Ions in Air, Hydrogen, and Nitrogen (these Proceedings, **4,** 1918, 91), the conclusion was reached that the so-called cluster hypothesis could no longer claim any reason for its existence and that the arguments for the small-ion theory should be considered conclusive.

It only remained for the small-ion theory to offer an adequate explanation for the difference between the positive and negative mobilities exhibited by all experimental results. This difference can easily be explained by the cluster hypothesis for if the ions were constituted by satellites of molecules surrounding single charges, the difference between the positive and negative mobilities could be ascribed to the difference between the number of constituent molecules in a positive and that in a negative ion. But with the small-ion theory such an explanation is not possible, since all ions are conceived of as single charged molecules.

In the aforementioned paper, an explanation for this experimental fact was offered on the basis of the Rutherford nucleus-atom theory. It was shown there that the difference between the two mobilities is due to the difference between the numbers of negative electrons in the two kinds of ions. There exists a greater attractive force between the positive ions and the uncharged molecules than that between the negative ions and the uncharged molecules on account of the fact that negative ions have more electrons than the positive. This results in a smaller mean free path, and hence a smaller mobility, for the positive ions than for the negative.

From this point of view it is evident that an excess of positive over negative mobility is scarcely to be expected in either gases or vapors. Since there has been very little work done on vapors and since some careful measurements of mobilities in them are necessary for the verification of this explanation in particular and of the small-ion theory in general, it was considered desirable to make some careful determinations in vapors, and hence the following work was undertaken.

*Method and Procedure.*—The method and procedure here employed were the same as those employed in the previous experiment. The only difference between this experiment and the former is that in the present one only low frequency alternating field was employed. This was because the vapors worked with required that pressures be sufficiently low for them to remain in the vaporized state, and at such pressures the high frequency high potential oscillating field proved inapplicable on account of the sparking across the gauze and the collecting plate. However, this difference does not at all effect the results, as the main purpose of employing the high potential field was to find out whether the mobilities would increase abnormally, and—since it had already been proved that they did not—the employment of the high potential field in the present experiment was entirely unnecessary.

Another difference between the present and the previous experiment is that in this one a different ionization chamber was used. This chamber was constructed on precisely the same plan as the former, but covered with a bell jar of about one-sixth the size of that covering the former apparatus. This last arrangement is more convenient in that it allowed the contents of the chamber to be evacuated and refilled with ease, and that the vapors were rendered as free of impurities as possible.

The vapors were produced by the same method as that employed by Wellisch.[1]

The measurements were made in the same manner as before, and the results calculated from the same formula.

*Results.*—The following tables show the results obtained for the various vapors used. It may be seen from these tables that the results for all these vapors at the various pressures are perfectly consistent with the law that the pressure times the mobility is constant, and that the mean values for the positive mobilities are—excepting in the case of $C_2H_5I$—smaller than those for the negative mobilities. In the case of $C_2H_5I$ the mean values may be considered the same. Therefore, the results of the present experiment are directly opposed to those obtained by Wellisch in 1909[1] in so far as the ratios of the negative to the positive mobilities are concerned.

In table 10 there is comparison between the results obtained by Wellisch and those obtained by me. It may be of interest to note that in 1912[2] Wellisch himself reversed his results for $C_2H_6O$, $C_5H_{12}$, and $SO_2$.

TABLE 1

SULPHUR DIOXIDE ($SO_2$)

| P | U+ | U− | K+ | K− | R |
|---|---|---|---|---|---|
| 60 | 5.327 | 5.547 | 0.421 | 0.437 | 1.04 |
| 70 | 4.523 | 4.523 | 0.416 | 0.416 | 1.00 |
| 80 | 4.935 | 4.035 | 0.423 | 0.423 | 1.00 |
| 90 | 3.329 | 3.371 | 0.394 | 0.399 | 1.01 |
| 105 | 2.833 | 2.895 | 0.398 | 0.407 | 1.02 |
| 120 | 2.669 | 2.561 | 0.421 | 0.404 | 0.96 |
| Mean.......... | | | 0.412 | 0.414 | 1.00 |

TABLE 2

ETHYL ALCOHOL ($C_2H_6O$)

| P | U+ | U− | K+ | K− | R |
|---|---|---|---|---|---|
| 15 | 19.728 | 19.024 | 0.389 | 0.378 | 0.97 |
| 20 | 14.017 | 14.017 | 0.369 | 0.369 | 1.00 |
| 25 | 11.097 | 11.097 | 0.365 | 0.365 | 1.00 |
| 30 | 9.513 | 9.864 | 0.375 | 0.388 | 1.03 |
| 40 | 6.053 | 7.008 | 0.319 | 0.367 | 1.15 |
| Mean.......... | | | 0.363 | 0.373 | 1.03 |

$P$ = pressure in mm. mercury. $U+$ = Positive mobility. $U-$ = Negative mobility. $K+$ = Positive mobility reduced to 760 mm. mercury. $K-$ = Negative mobility reduced to 760 mm. mercury. $R = K-/K+$.

TABLE 3

ALDEHYDE ($C_2H_4O$)

| P | U+ | U− | K+ | K− | R |
|---|---|---|---|---|---|
| 30 | 7.885 | 8.560 | 0.311 | 0.338 | 1.09 |
| 34 | | 7.885 | | 0.352 | |
| 38 | 5.993 | | 0.300 | | |
| 43 | 5.256 | 5.548 | 0.297 | 0.313 | 1.07 |
| 45 | 5.166 | 5.350 | 0.306 | 0.317 | 1.05 |
| 50 | 4.832 | 5.078 | 0.318 | 0.334 | 1.05 |
| 55 | 4.280 | 4.610 | 0.310 | 0.333 | 1.07 |
| Mean.......... | | | 0.307 | 0.331 | 1.07 |

TABLE 4

PENTANE ($C_5H_{12}$)

| P | U+ | U− | K+ | K− | R |
|---|---|---|---|---|---|
| 30 | 8.812 | 12.483 | 0.347 | 0.493 | 1.42 |
| 40 | 7.491 | 8.812 | 0.394 | 0.462 | 1.18 |
| 50 | 5.993 | 6.968 | 0.397 | 0.445 | 1.12 |
| 50 | 5.993 | 6.968 | 0.397 | 0.445 | 1.12 |
| 60 | 4.832 | 5.448 | 0.382 | 0.430 | 1.12 |
| 66 | 4.540 | 4.994 | 0.394 | 0.434 | 1.10 |
| Mean.......... | | | 0.385 | 0.451 | 1.17 |

TABLE 5

ETHYL CHLORIDE ($C_2H_5Cl$)

| P | U+ | U− | K+ | K− | R |
|---|---|---|---|---|---|
| 30 | 7.683 | 7.885 | 0.303 | 0.311 | 1.02 |
| 40 | 5.762 | 5.993 | 0.303 | 0.317 | 1.05 |
| 40 | 5.549 | 5.762 | 0.292 | 0.303 | 1.04 |
| 50 | 4.756 | 5.078 | 0.313 | 0.334 | 1.07 |
| 50 | 4.681 | 4.833 | 0.308 | 0.318 | 1.03 |
| Mean.......... | | | 0.304 | 0.317 | 1.04 |

TABLE 6

ACETONE ($C_3H_6O$)

| P | U+ | U− | K+ | K− | R |
|---|---|---|---|---|---|
| 50 | 3.699 | 4.035 | 0.243 | 0.265 | 1.09 |
| 60 | 2.959 | 3.131 | 0.235 | 0.247 | 1.05 |
| 64 | 2.833 | 2.959 | 0.239 | 0.250 | 1.07 |
| 70 | 2.513 | 2.665 | 0.229 | 0.245 | 1.07 |
| 76 | 2.336 | 2.421 | 0.234 | 0.242 | 1.03 |
| 80 | 2.219 | 2.296 | 0.234 | 0.242 | 1.03 |
| Mean.......... | | | 0.236 | 0.247 | 1.04 |

TABLE 7

ETHYL ACETATE ($C_4H_8O_2$)

| $P$ | $U+$ | $U-$ | $K+$ | $K-$ | $R$ |
|---|---|---|---|---|---|
| 50 | 3.309 | 3.648 | 0.219 | 0.240 | 1.09 |
| 60 | 2.567 | 3.167 | 0.229 | 0.250 | 1.09 |
| 65 | 2.701 | 2.900 | 0.231 | 0.248 | 1.07 |
| 70 | 2.421 | 2.663 | 0.223 | 0.245 | 1.09 |
| 75 | 2.290 | 2.533 | 0.226 | 0.250 | 1.10 |
| 80 | 2.172 | 2.334 | 0.230 | 0.246 | 1.07 |
| Mean.......... | | | 0.226 | 0.247 | 1.09 |

TABLE 8

ETHYL IODIDE ($C_2H_5I$)

| $P$ | $U+$ | $U-$ | $K+$ | $K-$ | $R$ |
|---|---|---|---|---|---|
| 30 | 5.166 | 4.833 | 0.203 | 0.191 | 0.94 |
| 35 | 4.104 | 4.161 | 0.189 | 0.192 | 1.01 |
| 50 | 2.466 | 2.512 | 0.162 | 0.169 | 1.04 |
| 60 | 2.336 | 2.296 | 0.184 | 0.181 | 0.98 |
| 60 | 2.141 | 2.166 | 0.169 | 0.171 | 1.01 |
| Mean.......... | | | 0.181 | 0.181 | 1.00 |

TABLE 9

METHYL IODIDE ($CH_3I$)

| $P$ | $U+$ | $U-$ | $K+$ | $K-$ | $R$ |
|---|---|---|---|---|---|
| 50 | 3.222 | 3.390 | 0.212 | 0.223 | 1.05 |
| 60 | 2.607 | 2.926 | 0.219 | 0.231 | 1.06 |
| 65 | 2.572 | 2.959 | 0.220 | 0.236 | 1.07 |
| 70 | 2.296 | 2.421 | 0.212 | 0.223 | 1.05 |
| 75 | 2.230 | 2.270 | 0.220 | 0.224 | 1.02 |
| 80 | 2.043 | 2.090 | 0.215 | 0.220 | 1.02 |
| Mean ........ | | | 0.216 | 0.226 | 1.05 |

TABLE 10

COMPARISON

| VAPOR | WELLISCH | | | | K.-L. YEN | |
|---|---|---|---|---|---|---|
| | 1909[1] | | 1915[2] | | 1917 | |
| | $K+$ | $K-$ | $K+$ | $K-$ | $K+$ | $K-$ |
| $C_2H_4O$ | 0.31 | 0.30 | | | 0.307 | 0.331 |
| $C_2H_6O$ | 0.34 | 0.27 | 0.39 | 0.412 | 0.363 | 0.373 |
| $C_3H_6O$ | 0.31 | 0.29 | | | 0.236 | 0.247 |
| $SO_2$ | 0.44 | 0.41 | 0.415 | 0.414 | 0.412 | 0.414 |
| $C_2H_5Cl$ | 0.33 | 0.31 | | | 0.304 | 0.317 |
| $C_5H_{12}$ | 0.36 | 0.35 | 0.370 | 0.440 | 0.385 | 0.451 |
| $C_4H_8O_2$ | 0.31 | 0.28 | | | 0.226 | 0.247 |
| $C_2H_5I$ | 0.17 | 0.16 | | | 0.181 | 0.181 |
| $CH_3I$ | 0.21 | 0.22 | 0.24 | 0.233 | 0.216 | 0.226 |

*Conclusion.*—From the results of the present experiment, it is evident that the apparent difficulty with the explanation proposed for the difference between the positive and negative mobilities does not really exist at all. It only remains for the exponents of the small-ion theory to deduce an exact formula for the mobility of ions on the basis of the Rutherford-Bohr theory from which we should expect that the peripheral negative electrons in the molecules and the ions would play the dominant if not indeed the only rôle in collisions.

I wish it to go on record that the present experiment was undertaken at the suggestion of Prof, R. A. Millikan, and under his direction; and also to thank Mr. W. R. Westhafer for his assistance in taking some of the measurements and computing some of the results.

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[1] *Phil. Trans. R. Soc., London,* (A), **209,** 1909, (249).

[2] *Phil. Mag., London,* **34,** 1917, (59).

# A CONTRIBUTION TO THE PETROGRAPHY OF THE SOUTH SEA ISLANDS

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A brief statement of the geological structure and general character of the rocks of the Islands of Tahiti, Moorea, and the Society Group has been given in a previous number of the Proceedings of the National Academy, from which it appears that each island is a profoundly eroded volcano, consisting mainly of basaltic lavas rich in olivine and augite, with inconspicuous feldspar, and that at five of the volcanic islands there are trachytic or phonolitic lavas which have been erupted late in the period of activity. In two volcanoes erosion has exposed coarsely crystallized cores of gabbroic and theralitic rocks with peridotites, and in one case syenites and nephelite-syenites as the latest eruptions.

Nearly seventy years ago J. D. Dana called attention to these syenitic rocks on Tahiti, and remarked that they were only a feldspathic variety of the same igneous rocks that constitute the island. Eight years ago Lacroix published a description of the alkalic rocks of Tahiti with chemical analyses, laying particular stress on the syenitic varieties and on the haüynophyres, with certain limburgitic lavas, but noting the fact that the preponderant rocks of the islands are basalts rich in olivine. From the emphasis laid upon the alkalic rocks, one gets the impression that they are more abundant than is actually the case. However, their theoretical importance has not been exaggerated. More recently Marshall has analysed and described phonolitic rocks from the Leeward Islands and from Raratonga, Cooks Islands, and has analyzed several rocks from Tahiti. So there are already a number of chemical analyses of igneous rocks from the islands of this part of the Pacific Ocean.

In order to extend the investigation somewhat further, and to include the more common varieties of basalt so as to give a clearer idea of the prevailing rocks of the islands, chemical analyses have been made of rocks from different islands of the Georgian and Society groups. These have been placed in sequence in one table for comparison with one another, and to show the resemblances among the various lavas of these islands. The analyses have been obtained in part with the aid of grants 192 and 203, from the Bache Fund of the Academy. That is, those by Professor Foote and by Dr. Washington. The microscopical study has also been carried on with the aid of these grants. As the specimens collected represent over 550 rocks from seven volcanic islands, it is only possible in this preliminary statement to notice particularly the 30 specimens whose analyses are published for the first time in the accompanying table, with some observations concerning their relations to the rocks with which they are associated.

The rocks of Tahiti are almost wholly basalts rich in olivine and augite, with few or no phenocrysts of feldspar. They differ from one another somewhat in the size and abundance of the phenocrysts of olivine and augite. At one extreme are basalts with abundant phenocrysts; at the other are basalts without any, but with ill-defined spots which are lighter colored than the body of the rock. Such basalts form large flows often in superposition, as at Point Tapahi on the north coast where a strongly porphyritic basalt forms the lower layer or flow, at the water's edge, and a non-porphyritic spotted basalt forms the upper flow. The upper rock has been analyzed, no. 19 in the table, and proves to be a limburgose, bordering on camptonose, with 8.5% of normative nephelite. The light-colored spots in the rock are probably due to areas of altered nephelite, or to analcite. The rock is aphanitic, and under the microscope is seen to be holocrystalline; composed of augite, magnetite, ilmenite, and olivine in a matrix of plagioclase with nephelite or analcite. Similar basalt forms a massive flow and has been quarried near Papeete. A strongly porphyritic basalt rich in olivine, like the rock at Point Tapahi, is in place on the road farther west. Its analysis, no. 27, shows it to be uvaldose, a highly mafic rock without normative nephelite. It is holocrystalline; the groundmass crowded with augite, olivine and magnetite, with quite subordinate amount of plagioclase feldspar. It is in fact limburgite. These two varieties of basalt are common throughout the islands visited, a very similar rock, no. 28, having been analyzed from the west coast of Raiatea. It is, however, somewhat more coarsely crystalline, the individual crystals being distinctly visible microscopically. Some of the plagioclase feldspars have outer zones of alkalic feldspar, probably soda-orthoclase.

A limburgitic basalt which appears to be a large massive body, exposed in Fautaua Valley on the trail above the waterfall, is a gray rock with small miarolitic cavities. It is holocrystalline and consists of abundant subhedral, violet-tinted augites, of variable sizes, with much colorless olivine, subhedral magnetite, in part dendritic, and areas of poikilitic plagioclase, with patches where the matrix is alkalic feldspar and analcite. The chemical analysis is no. 26 and the norm is relatively high in alkalic feldspar, low in anorthite, with 7% of normative nephelite. There is 5% of apatite which appears as thin acicular prisms. A corresponding amount of apatite occurs in a theralite from Taiarapu, analysis 22. Another basalt from Fautaua Valley is found in boulders in the stream near the ridge above the falls. Its chemical composition, no. 25, is similar to the rock just described, no. 26, but it appears somewhat differently under the microscope. It consists chiefly of violet-tinted augite, with abundant colorless olivine, and magnetite, and clusters of rods of ilmenite, with minute needles of augite. Between these is a small amount of colorless matrix which is in part plagioclase feldspar. The norm shows a small amount of anorthite, 19% of feldspathoids, and no alkalic feldspar. There is about 2% of calcium orthosilicate which does not appear as melilite in the mode, and is probably incorporated in the mafic minerals. This is also a limburgite.

A basalt still richer in olivine, of the utmost freshness occurs on the east side of Raiatea, its analysis, no. 24, shows 23.97% of magnesia, or nearly 45% of normative olivine. It is a most beautiful chrysophyre, or limburgite, with a holocrystalline groundmass crowded with microscopic augites, olivines, magnetite and subordinate plagioclase. On the Island of Moorea a limburgite with similar chemical composition, no. 23, was found in large blocks, but not in place. It is more coarsely crystallized and resembles a peridotite megascopically. It is perfectly fresh and has a scant matrix of microscopic plagioclase. The norms of these highly olivinitic basalts do not contain normative nephelite, but contain normative hypersthene. The silica is comparatively high for the amount of alumina, which is low; the magnesia being abnormally high.

In the central core of the dissected volcano of Tahiti, and in that of Taiarapu, there are coarse-grained theralites which are chemically similar to the non-porphyritic basalt forming the upper flow at Point Tapahi, Tahiti. Their analyses are nos. 20, 21, and 22. They are limburgose and limburgose-etindose, and are characterized by notable amounts of normative nephelite, which is also modal. In no. 22 there is also normative leucite, which, however, does not appear in the mode, which contains considerable biotite. The theralite from Vaitipihia, Tautira Valley, Taiarapu, no. 20, is rich in augite, brown amphibole and biotite, with subordinate plagioclase and nephelite. That from Maroto River, in the Papenoo Valley, Tahiti, no. 21, is very much like no. 20, but has less amphibole and a slightly different texture. The other theralite from Vaitipihia, no. 22, contains large brown amphiboles, surrounded by brown biotite, with little augite, and much apatite. The chemical compositions of coarse-grained rocks of unusual mineral composition from the core of Taiarapu are given in nos. 29 and 30. The first is a narrow vein of pyroxenic rock traversing basalt. It consists almost wholly of slender dendritic crystals of augite in a microscopically fine-grained matrix. The norm has 45% of diopside, 5% of olivine, 26% of anorthite and small amounts of normative nephelite and leucite. The second rock, no. 30, is a peridotite composed of augite, olivine, brown hornblende, iron ores and pyrite, with very small amounts of feldspathic minerals; the norm containing a little normative nephelite and leucite.

The analyses of other basalts from these islands are given in nos. 14 to 18 in the table. They are camptonose and camptonose-auvergnose, and are from Moorea, Huahine, Tahaa and Bora Bora. The basalt from Tahaa, no. 14, is rather coarse-grained and forms a dike near the coast on Rei Point. It is the rock used for ballast by small boats in this region, and may be found in the ports on various islands, and is probably the rock called granite by early explorers. It is a dolerite and looks like a fine-grained gabbro. It is rich in olivine and its norm contains a small amount of nephelite. The basalt from Bora Bora, no. 15, is porphyritic, very rich in olivine, and has nearly the same chemical composition as no. 14. The columnar basalt from Moorea, no. 17, and the basalt from Huahine, no. 16, are very similar chemically, while the basalt from the base of Mauratapu on Huahine, no. 18, is lower in magnesia and higher in lime and alumina. These basalts do not contain normative nephelite, and differ from one another somewhat in texture.

From the foregoing it is seen that the basaltic rocks of this region, and their coarsely crystallized phases, which occur in the cores of the volcanoes of Tahiti and Taiarapu, are normatively nephelite-bearing, except some of the highly olivinitic varieties, and some others. Nephelite is visibly present in the modes of the coarsely crystallized rocks, and is possibly present in microscopic crystals in many of the fine-grained and aphanitic basalts, though it is probable that it is represented by analcite in some instances, either as a primary mineral, or as a product of alteration.

The trachytic and phonolitic lavas, which are known to occur at five of the volcanic islands visited, are very similar to one another chemically, as is shown by analyses 1 to 8. The rocks from Nutae, no. 3, and Point Riri, no. 6 on Taiarapu, are light gray and but slightly porphyritic, with fissile parting and satin lustre. The first occurs as boulders on the beach associated with haüynophyre. The second is in place, and is exposed in large blocks. Each contains a small amount of normative nephelite, and a little that can be identified as modal nephelite, so that the rocks are properly nephelite-bearing trachytes, rather than phonolites. They have a microtrachytic texture, the first one containing small scattered phenocrysts of alkalic feldspar. Similar rocks occur on Huahine, nos. 1 and 5. They are darker colored and more fissile. Microscopically they appear to contain abundant minute crystals of nephelite, and to be characteristic phonolites. However, most of the rectangular crystals are alkalic feldspars and not nephelite, as their index of refraction shows. These rocks also are nephelite-bearing trachytes and not properly phonolites. The same is true of similar massive rocks from Raiatea, the fissile mass forming the top of Mount Tapioi, no. 4, and the rock of the sugar-loaf dome on the east side of the island, no. 2. These rocks are nephelite-bearing trachytes with small amounts of nephelite. The rock from the top of Mount Tapioi contains minute crystals of what appear to be sodalite scattered through the feldspars. Similar nephelite-bearing trachytes with less nephelite form large bodies of rock on Moorea, the analysis of one of them being given in no. 8.

Some varieties of these trachytic rocks, occurring on Taiarapu, contain haüynite in small crystals, and 12% of normative nephelite and are properly phonolites. Analysis 7 is from such a rock. One variety, no. 11, from the beach at Tautira consists of alkalic feldspar and andesine, with some nephelite and sodalite, and contains abundant small phenocrysts of brown hornblende, with much titanite and few augites and micas. Chemically it is very similar to a tephritic trachyte from Bauza, Columbreta, described by Becke. Its symbol in the Quantitative System of Classification shows that it is transitional. In the qualitative system it corresponds to a nephelite-latite, or

TABLE OF CHEMICAL ANALYSES AND NORMS OF LAVAS FROM SOUTH PACIFIC ISLANDS

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| $SiO_2$ | 61.98 | 62.20 | 61.07 | 62.44 | 61.90 | 60.48 | 58.84 | 57.61 | 56.33 | 56.68 |
| $Al_2O_3$ | 18.72 | 17.85 | 17.77 | 18.87 | 18.37 | 19.35 | 20.30 | 16.47 | 18.44 | 18.73 |
| $Fe_2O_3$ | 2.34 | 2.31 | 2.18 | 1.87 | 2.46 | 2.34 | 2.74 | 2.84 | 5.89 | 4.34 |
| FeO | 1.17 | 0.95 | 1.49 | 0.87 | 0.66 | 1.19 | 0.64 | 4.72 | 2.60 | 0.94 |
| MgO | 0.39 | 0.83 | 0.88 | 0.62 | 0.46 | 0.75 | 0.60 | 0.86 | 1.04 | 1.97 |
| CaO | 1.09 | 1.33 | 1.30 | 1.35 | 0.58 | 1.68 | 1.66 | 2.61 | 2.70 | 3.60 |
| $Na_2O$ | 7.16 | 6.74 | 6.80 | 7.29 | 7.95 | 6.50 | 7.48 | 5.06 | 4.34 | 5.64 |
| $K_2O$ | 5.66 | 5.80 | 5.57 | 5.66 | 5.36 | 5.90 | 5.72 | 5.97 | 4.82 | 3.85 |
| $H_2O+$ | 1.05 | 0.95 | 1.71 | 0.32 | 1.52 | 0.87 | 0.68 | 1.43 | 1.85 | 1.71 |
| $H_2O-$ | 0.42 | 0.36 | 0.11 | 0.17 | 0.61 | 0.25 | 0.31 | 0.35 | 0.52 | 0.80 |
| $TiO_2$ | 0.26 | 0.57 | 0.94 | 0.62 | 0.20 | 0.72 | 0.72 | 0.51 | 0.88 | 0.91 |
| $ZrO_2$ | | | 0.12 | | | | | 0.00 | | |
| $CO_2$ | | | none | | | | | 0.03 | | |
| $P_2O_5$ | 0.03 | 0.14 | tr. | 0.09 | 0.01 | 0.16 | 0.13 | 0.42 | 0.61 | 0.74 |
| Cl | | | | | | | | 0.08 | | |
| F | | | | | | | | n. f. | | |
| S | | | none | | | | | 0.04 | | |
| $Cr_2O_3$ | tr. | tr. | | tr. | tr. | tr. | 0.02 | 0.00 | 0.00 | tr. |
| MnO | 0.30 | 0.24 | 0.05 | 0.21 | 0.26 | 0.14 | 0.12 | 1.39 | 0.11 | 0.33 |
| BaO | | | none | | | 0.14 | 0.07 | 1.02 | | |
| SrO | | | | | | | 0.00 | 0.01 | | |
| | 100.57 | 100.27 | 99.99 | 100.38 | 100.34 | 100.47 | 100.03 | 100.42 | 100.13 | 100.24 |

*Norms*

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| Q | | | | | | | | | 7.02 | 0.60 |
| or | 33.92 | 34.47 | 33.36 | 33.92 | 31.69 | 35.03 | 33.92 | 35.58 | 28.36 | 22.80 |
| ab | 51.87 | 52.92 | 50.83 | 51.87 | 52.40 | 46.63 | 41.39 | 40.35 | 36.68 | 47.68 |
| an | 1.67 | 1.11 | 1.39 | 1.67 | | 6.12 | 4.73 | 4.45 | 9.73 | 13.07 |
| ne | 4.83 | 2.27 | 3.69 | 5.40 | 6.53 | 4.54 | 11.93 | 1.42 | | |
| C | | | | | | | | | 2.45 | 0.41 |
| Z | | 0.18 | | | | | | | | |
| ac | | | | | 2.31 | | | | | |
| di | 2.65 | 3.67 | 3.89 | 3.34 | 2.48 | 1.08 | 2.16 | 4.80 | | |
| wo | 0.23 | | | | | | | | | |
| hy | | | | | | | | | 2.60 | 4.90 |
| ol | | 0.28 | 0.28 | | | 0.98 | 0.35 | 5.81 | | |
| mt | 3.25 | 2.32 | 2.09 | 1.62 | 2.31 | 2.32 | 0.23 | 4.18 | 6.03 | 1.62 |
| il | 0.61 | 1.06 | 1.82 | 1.22 | 0.46 | 1.37 | 1.37 | 0.91 | 1.67 | 1.67 |
| hm | | 0.64 | 0.80 | 0.80 | 0.16 | 0.64 | 2.56 | | 1.76 | 3.20 |
| ap | | 0.34 | | 0.34 | | 0.34 | 0.34 | 1.01 | 1.34 | 1.68 |
| etc | 1.50 | 1.31 | 1.87 | 0.49 | 2.14 | 1.26 | 1.08 | 1.96 | 2.37 | 2.51 |
| | 100.53 | 100.39 | 100.20 | 100.67 | 100.49 | 100.31 | 100.06 | 100.47 | 100.01 | 100.14 |

1. I. 5.1. (3)4. phlegrose-nordmarkose, nephelite-trachyte, road S. of Mt. Paeo, Huahine.
2. I′. 5.1. (3)4. phlegrose-nordmarkose, nephelite-trachyte, E. base of Sugarloaf peak, Raiatea.
3. I′. 5.1. (3)4. phlegrose-nordmarkose, nephelite-trachyte, Nutae, Taiarapu, Tahiti.
4. I′. 5.1. ′4. nordmarkose, nephelite-trachyte, Mount Tapioi, Raiatea.
5. I′. 5.1. ′4. nordmarkose, nephelite-trachyte, S. W. base of Mauratapu, Huahine.
6. I′. 5.1(2). 3–4. phlegrose-nordmarkose, nephelite-trachyte, Point Riri, Taiarapu, Tahiti.
7. I′. (5)6.1′. ′4. nordmarkose-miaskose, phonolite, Vaitia, Tautira valley, Taiarapu.
8. ′II. 5.1(2). 3. monzonose-ilmenose, nephelite-trachyte, Papetoai valley, Moorea.
9. (I)II. ′5.2. 3′. pulaskose-monzonase, latite, Road, 1 km. E. of Papetoai, Moorea.
10. (I)II. 5.2. 4. larvikose-akerose, kohalaite, Second spur W. of Mt. Tapioi, Raiatea.

Nos. 1, 2, 4, 5, 6, 7, 9, 10 by H. W. Foote; No. 3 by H. S. Washington; No. 8 by E. W. Morley.

TABLE OF CHEMICAL ANALYSES AND NORMS OF LAVA FROM SOUTH PACIFIC ISLANDS

| | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 |
|---|---|---|---|---|---|---|---|---|---|---|
| $SiO_2$ | 52.11 | 50.11 | 50.73 | 47.60 | 46.55 | 46.96 | 46.01 | 47.55 | 44.74 | 42.46 |
| $Al_2O_3$ | 20.04 | 18.91 | 17.22 | 13.35 | 10.75 | 11.00 | 11.49 | 14.53 | 16.74 | 13.85 |
| $Fe_2O_3$ | 3.27 | 4.55 | 3.62 | 2.83 | 2.66 | 2.22 | 2.27 | 2.23 | 3.70 | 5.21 |
| $FeO$ | 2.15 | 2.51 | 3.74 | 8.02 | 9.49 | 9.42 | 10.17 | 7.53 | 8.53 | 7.58 |
| $MgO$ | 1.50 | 2.49 | 2.72 | 10.86 | 13.39 | 14.21 | 12.72 | 7.01 | 4.80 | 7.76 |
| $CaO$ | 3.90 | 4.70 | 5.66 | 9.47 | 8.72 | 9.46 | 9.50 | 11.13 | 9.88 | 11.28 |
| $Na_2O$ | 5.72 | 7.47 | 7.92 | 3.00 | 2.76 | 1.64 | 1.82 | 2.14 | 4.42 | 3.67 |
| $K_2O$ | 5.30 | 4.29 | 3.89 | 1.82 | 1.56 | 1.40 | 1.38 | 1.84 | 1.14 | 2.34 |
| $H_2O+$ | 2.84 | 0.69 | 0.36 | 0.43 | 0.87 | 0.61 | 1.10 | 1.68 | 1.12 | 0.65 |
| $H_2O-$ | 0.30 | 0.78 | 0.12 | 0.17 | 0.51 | 0.05 | 0.33 | 0.57 | 0.33 | 0.07 |
| $TiO_2$ | 1.90 | 1.97 | 1.91 | 2.65 | 2.54 | 2.77 | 2.90 | 3.24 | 3.68 | 3.64 |
| $ZrO_2$ | | 0.01 | 0.00 | | | | | | | |
| $CO_2$ | | 0.02 | 0.16 | | | | | | | |
| $P_2O_5$ | 0.40 | 0.55 | 0.76 | 0.33 | 0.36 | 0.38 | 0.36 | 0.49 | 0.70 | 0.90 |
| Cl | | 0.12 | 0.28 | | | | | | | |
| F | | 0.13 | 0.14 | | | | | | | |
| S | | 0.23 | 0.29 | | | | | | | |
| $Cr_2O_3$ | 0.00 | 0.01 | 0.00 | 0.06 | 0.09 | 0.08 | 0.11 | 0.01 | tr. | 0.05 |
| $MnO$ | 0.14 | 0.65 | 0.35 | 0.13 | 0.16 | 0.16 | 0.18 | 0.13 | 0.19 | 0.19 |
| $BaO$ | | 0.08 | 0.18 | | | | | | | |
| $SrO$ | | 0.10 | 0.22 | | | | | | | |
| | 99.57 | 100.37 | 100.27 | 100.27 | 100.41 | 100.36 | 100.34 | 100.08 | 99.97 | 99.65 |

*Norms*

| | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 |
|---|---|---|---|---|---|---|---|---|---|---|
| or | 31.14 | 25.58 | 22.80 | 10.56 | 9.45 | 8.34 | 8.34 | 10.56 | 6.67 | 13.90 |
| ab | 28.30 | 22.01 | 24.10 | 18.86 | 17.82 | 13.62 | 15.20 | 17.82 | 21.48 | 3.67 |
| an | 13.34 | 5.28 | 0.28 | 17.79 | 12.23 | 18.63 | 19.18 | 24.74 | 22.52 | 14.46 |
| ne | 10.79 | 22.15 | 23.00 | 3.41 | 3.12 | | | | 8.52 | 14.77 |
| di | 2.59 | 11.66 | 17.17 | 21.75 | 22.49 | 20.41 | 19.65 | 21.69 | 17.83 | 28.66 |
| wo | | | 0.58 | | | | | | | |
| hy | | | | | | 9.74 | 5.78 | 8.14 | | |
| ol | 1.82 | 0.56 | | 17.72 | 24.27 | 19.35 | 20.88 | 4.16 | 7.41 | 7.07 |
| mt | 1.86 | 4.41 | 5.34 | 4.41 | 4.18 | 3.48 | 3.25 | 3.25 | 5.34 | 7.66 |
| il | 3.65 | 3.80 | 3.65 | 5.17 | 4.86 | 5.32 | 5.47 | 6.08 | 6.99 | 6.84 |
| hm | 2.08 | 1.60 | | | | | | | | |
| ap | 1.01 | 1.34 | 2.02 | 0.67 | 1.01 | 1.01 | 1.34 | 1.34 | 1.68 | 2.02 |
| etc | 3.14 | 2.07 | 1.35 | 0.60 | 1.38 | 0.66 | 1.54 | 2.26 | 1.45 | 0.77 |
| | 99.72 | 100.46 | 100.29 | 100.94 | 100.81 | 100.56 | 100.63 | 100.04 | 99.89 | 99.82 |

11. (I)II. (5)6. 2. 3(4). essexose-borolanose, tautirite Beach, Tautira, Taiarapu, H. W. Foote.
12. II. 6. 1(2) 4. essexose-lardalose, haüynophyre, Faurahi valley, Mataia, Tahiti, E. W. Morley.
13. II. 6′. 1. 4. lardalose, haüpnophyre, Ururoa valley, Tahiti, E. W. Morley.
14. III. 5′. 3. 4. camptonose, dolerite, Dike, Rei Point, Tahaa, H. W. Foote.
15. III′, 5′. ′3. 4. camptonose, basalt, Central mountain, Bora Bora, H. W. Foote.
16. III′. 5. 3(4). (3)4. kentallenose-camptonose, basalt, South of Fare, Huahine, H. W. Foote.
17. III′. 5, 3(4). ′4. auvergnose-camptonose, basalt, Mountain between bays, Moorea, H. W. Foote.
18. III. 5. (3)4. (3)4. camptonose-auvergnose, basalt, S. W. base of Mauratapu, Huahine, H. W. Foote.
19. (II)III. (5)6. 3. 4(5). camptonose-limburgose, basalt, Upper flow, Point Tapahi, Tahiti, H. W. Foote.
20. III. 6′. (2)3. 4. monchiquose-limburgose, theralite, Vaitipihia, Tautira, Taiarapu, H. W. Foote.

TABLE OF CHEMICAL ANALYSES AND NORMS OF LAVA FROM SOUTH PACIFIC ISLANDS

| | 21 | 22 | 23 | 24 | 25 | 26 | 27 | 28 | 29 | 30 |
|---|---|---|---|---|---|---|---|---|---|---|
| $SiO_2$ | 41.70 | 37.59 | 43.86 | 43.45 | 42.78 | 43.05 | 43.46 | 43.29 | 40.28 | 39.68 |
| $Al_2O_3$ | 14.55 | 15.18 | 7.42 | 7.21 | 9.26 | 9.21 | 8.69 | 9.10 | 10.68 | 7.35 |
| $Fe_2O_3$ | 6.11 | 3.98 | 2.41 | 3.24 | 2.00 | 1.90 | 4.17 | 6.88 | 5.23 | 7.81 |
| $FeO$ | 7.83 | 6.50 | 12.39 | 9.92 | 10.82 | 10.21 | 8.58 | 7.25 | 8.37 | 8.60 |
| $MgO$ | 7.80 | 8.49 | 21.53 | 23.97 | 12.44 | 13.41 | 18.82 | 16.21 | 10.19 | 14.29 |
| $CaO$ | 12.39 | 12.73 | 7.96 | 6.85 | 14.37 | 10.15 | 10.41 | 10.11 | 17.23 | 15.78 |
| $Na_2O$ | 2.92 | 2.80 | 1.16 | 1.28 | 2.89 | 3.58 | 1.02 | 1.44 | 0.47 | 0.82 |
| $K_2O$ | 1.80 | 2.38 | 0.66 | 0.68 | 1.24 | 0.73 | 0.54 | 0.88 | 0.41 | 0.47 |
| $H_2O+$ | 0.73 | 2.44 | 0.45 | 0.70 | 0.40 | 0.87 | 1.01 | 1.48 | 2.08 | 0.39 |
| $H_2O-$ | 0.01 | 0.13 | 0.18 | 0.36 | 0.05 | 0.90 | 0.42 | 0.54 | 0.39 | 0.01 |
| $TiO_2$ | 3.46 | 5.01 | 2.10 | 2.03 | 2.66 | 3.00 | 2.34 | 2.84 | 4.82 | 4.50 |
| $ZrO_2$ | | | | | 0.02 | 0.00 | | | | |
| $CO_2$ | | | | | 0.01 | 0.10 | | | | |
| $P_2O_5$ | 0.63 | 2.29 | 0.19 | 0.24 | 0.50 | 2.21 | 0.28 | 0.32 | 0.27 | 0.17 |
| $Cl$ | | | | | 0.07 | 0.13 | | | | |
| $F$ | | | | | 0.03 | 0.03 | | | | |
| $S$ | | | | | 0.24 | 0.24 | | | | |
| $Cr_2O_3$ | 0.03 | 0.00 | 0.08 | 0.17 | 0.12 | 0.10 | 0.17 | 0.11 | 0.02 | 0.09 |
| $MnO$ | 0.22 | 0.13 | 0.18 | 0.17 | 0.43 | 0.36 | 0.15 | 0.14 | 0.13 | 0.21 |
| $BaO$ | | | | | 0.04 | 0.05 | | | | |
| $SrO$ | | | | | 0.06 | 0.00 | | | | |
| | 100.18 | 99.65 | 100.57 | 100.27 | 100.43 | 100.23 | 100.06 | 100.59 | 100.57 | 100.17 |

*Norms*

| | 21 | 22 | 23 | 24 | 25 | 26 | 27 | 28 | 29 | 30 |
|---|---|---|---|---|---|---|---|---|---|---|
| or | 10.56 | 6.67 | 3.89 | 3.89 | | 4.45 | 2.78 | 5.56 | 0.56 | |
| ab | 3.67 | | 9.96 | 10.48 | | 16.77 | 8.38 | 12.05 | | |
| an | 21.41 | 21.68 | 10.29 | 12.23 | 8.62 | 7.23 | 17.79 | 15.57 | 26.13 | 15.29 |
| ne | 11.36 | 12.78 | | | 13.35 | 7.38 | | | 1.99 | 3.69 |
| lc | | 6.10 | | | 5.67 | | | | 1.31 | 2.18 |
| di | 28.84 | 21.01 | 22.67 | 17.82 | 42.61 | 23.42 | 25.29 | 25.59 | 45.32 | 43.46 |
| wo | | | | | | | | | | 2.42 |
| hp | | | 2.72 | 1.16 | | | 6.06 | 4.50 | | |
| ol | 6.89 | 8.39 | 41.55 | 44.79 | 17.92 | 25.15 | 26.89 | 19.21 | 5.40 | 12.47 |
| cs | | | | | 1.89 | | | | | |
| mt | 8.82 | 5.80 | 3.48 | 4.64 | 3.25 | 3.02 | 6.03 | 10.21 | 7.66 | 11.37 |
| il | 6.69 | 9.58 | 3.95 | 3.80 | 5.17 | 5.78 | 4.41 | 5.32 | 9.12 | 8.51 |
| ap | 1.34 | 5.38 | 0.34 | 0.34 | 1.34 | 5.04 | 0.67 | 0.67 | 0.67 | 0.34 |
| etc | 0.77 | 2.58 | 0.71 | 1.23 | 0.92 | 2.32 | 1.60 | 2.02 | 2.49 | 0.49 |
| | 100.35 | 99.97 | 99.56 | 100.38 | 100.74 | 100.56 | 99.90 | 100.70 | 100.65 | 100.22 |

21. III. 6. 3. 4. limburgose, theralite, Maroto river, Papenoo valley, Tahiti, H. W. Foote.
22. III. (6)7. 3. (3)4. limburgose-etindose, theralite, Vaitipihia, Tautira, Taiarapu, H. W. Foote.
23. IV. 1(2). 3(4). (1)2. 2. wehrlose-rossweinose, limburgite, Papetoai valley, Moorea, H. W. Foote.
24. IV. 1(2). 4. (1)2. 2. limburgite, Spur E. of Sugar-loaf peak, Raiatea, H. W. Foote.
25. IV. 1(2). 2'. 2'. 2'. montrealose-palisadose, limburgite, Fautaua valley, above falls, Tahiti, E. W. Morley.
26. (III)IV. (1)2. 2. 3. 2. 2. rossweinose-uvaldose, limburgite, Fautaua valley, trail above falls, Tahiti, E. W. Morley.
27. IV. '2, 3, '2. 2. uvaldose, limburgite, on road W. of Point Tapahi, Tahiti, H. W. Foote.
28. 'IV. 2. (2)3. '2. 2. uvaldose limburgite, Spur N. of bay, W. side of Raiatea, H. W. Foote.
29. IV. 2. 1(2). 2(3). 2. brandbergose-yamaskose, augitic vein, Tautira valley, Taiarapu, H. W. Foote.
30. IV. 2. 2. 3'. 2. paolose, peridotite, Vaitipihia, Tautira valley, H. W. Foote.

tephritic trachyte, or to Lacroix's nephelite-micromonzonite. It is proposed to call this particular variety of rock, *tautirite*, from the valley in which it occurs.

On Tahiti, in the valley of Ururoa on the north coast, there is a variety of haüynophyre with microscopic haüynites, analysis, 13, which is chemically somewhat like the haüynophyres analyzed and described by Lacroix, and by Marshall. The microcrystalline groundmass consists of alkalic feldspar, augite and magnetite, with small phenocrysts of haüynite, augite and very few brown hornblendes. A similar haüynophyre occurs sparingly in the valley of Faurahi, on the southwest side of Tahiti. Its chemical analysis is no. 12. These rocks are scarce on Tahiti.

On Raiatea the heavy sheet of trachytic lava, which tops the ridge and spurs of the northern half of the island, varies somewhat in composition in different places. On the second spur west of Mount Tapioi it has numerous phenocrysts of feldspar, with fewer of mica and paramorphs of hornblende. The chemical analysis, no. 10, shows it is kohalaite, or oligoclase-trachyte. A non-porphyritic gray lava on Moorea is unusual in appearance for rocks of this region. Its chemical analysis, no. 9, shows it is latite, an aphanitic lava phase of monzonite. The corresponding monzonite occurs as a variety of the syenitic rocks in the core of the Tahitian volcano.

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# *THE LAW CONTROLLING THE QUANTITY AND RATE OF REGENERATION*

By Jacques Loeb

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1. It is well known that isolated pieces of a plant or a lower animal may regenerate into a whole organism again. In order to replace the current vague speculations concerning this phenomenon by a scientific theory in the sense of the physicist, quantitative experiments are required. The writer has for the past two years made such experiments which have led to a remarkably simple law controlling the quantity of regeneration in an isolated piece of an organism. This law can be expressed as follows: The mass of tissue regenerated by an isolated piece of an organism is under equal conditions and in equal time in direct proportion to the mass of growth material contained in the sap (or blood) of the isolated piece. The experiments on which this law is based were carried out on an organism unusually favorable for investigations of this kind, namely, the plant *Bryophyllum calycinum* (known to many laymen as the Bermuda 'life plant'). When leaves of this plant are isolated from the stem they will regenerate shoots in some or many of their notches. If a piece of

stem is cut out from a plant it will form shoots from its two most apical buds. My experiments have yielded the result that the mass of shoots formed in the latter case is in direct proportion to the mass of a leaf attached to the stem; and to the mass of the isolated leaf in the former case. The data concerning regeneration in an isolated leaf have already been published[1] but will be repeated here to show the identity of the law in both cases.

2. When we cut out two sister leaves of *Bryophyllum*. *i.e.*, a pair of leaves taken from the same node of a plant, and keep them under the same condition of moisture, temperature, and light, the two sister leaves possessing equal mass will produce approximately equal masses of shoots in equal times, although the number of shoots produced by the two sister leaves may vary considerably (table 1).

TABLE 1

INFLUENCE OF MASS OF LEAVES UPON MASS OF SHOOTS REGENERATED BY LEAF

| | | WEIGHT OF LEAVES | NUMBER OF SHOOTS | WEIGHT OF SHOOTS | MILLIGRAMS OF SHOOTS PRODUCED PER GRAM OF LEAF |
|---|---|---|---|---|---|
| | | *grams* | | *grams* | |
| Experiment I. Duration, 22 days. . | 8 leaves | 16.430 | 37 | 1.675 | 102 |
| | 8 sister leaves | 16.476 | 40 | 1.682 | 102 |
| Experiment II. Duration, 29 days. . | 9 leaves | 12.022 | 24 | 1.436 | 119 |
| | 9 sister leaves | 11.861 | 20 | 1.348 | 114 |
| Experiment III. Duration, 30 days. . . | 12 leaves, intact | 18.435 | 25 | 2.884 | 156 |
| | 12 sister leaves, each cut into 4 pieces | 17.070 | 50 | 2.747 | 161 |

When we reduce the mass of one set of the sister leaves (by cutting away parts of the leaf), while that of the other set remains intact, both sets of leaves will produce in equal time and under equal conditions shoots whose masses are approximately proportional to the masses of the two sets of leaves (table 2).

From this it follows that equal masses of leaves produce equal masses of shoots, regardless of the number of shoots. Since chemical substances (water and solutes) are the only factors among those to be considered here which can vary in direct proportion with the mass of the leaves, it follows that the quantity of shoot formation in an isolated leaf is determined by the quantity of certain material contained in the sap of the leaf. This material is probably the usual material required for growth: water, and certain solutes, sugar, amino acids, salts, etc.

3. It was necessary to test the validity of this law for the regeneration of shoots in isolated stems. The facts just mentioned suggested the method required to yield rational quantitative results. This method consisted in the measurement of the influence of the mass of a leaf attached to a piece of stem upon the quantity of shoot formation in the latter. In order to obtain strictly comparable results, it was necessary again to compare the effect of sister leaves, since sister leaves alone are sufficiently alike to guarantee comparable results. The method of procedure was as follows. Stems of *Bryophyllum* containing three nodes and one pair of leaves in the third (most basal) node were split longitudinally into two halves, each half containing one leaf. One leaf remained intact, while the sister leaf attached to the other half of the stem was reduced in size by cutting away the greater part. Six whole stems were used for one experiment. After splitting, the halved stems were suspended in an aquarium with the apices of the leaves just dipping in water. Each half stem formed one new shoot from the apical bud and new roots at the base, but regeneration started earlier in the half stems with a whole leaf attached than in the half stems with a leaf reduced in size. After about five weeks the regenerated shoots were cut off and weighed. It was found that *the mass of the shoots regenerated in the two sets of halved stems was in exact proportion to the mass of the leaves attached to the stems* (table 3).

TABLE 2

INFLUENCE OF MASS OF LEAVES UPON MASS OF SHOOTS REGENERATED BY LEAF

| | | WEIGHT OF LEAVES | NUMBER OF SHOOTS | WEIGHT OF SHOOTS | MILLIGRAMS OF SHOOTS PRODUCED PER GRAM OF LEAF |
|---|---|---|---|---|---|
| | | *grams* | | *grams* | |
| Experiment I. Duration 37 days.......... | 5 leaves, with center cut out | 7.610 | 11 | 0.755 | 99 |
| | 5 sister leaves, intact | 13.800 | 9 | 1.405 | 101 |
| Experiment II. Duration, 25 days......... | 7 leaves, with center cut out | 9.899 | 21 | 1.213 | 122 |
| | 7 sister leaves, intact | 16.935 | 25 | 1.995 | 118 |
| Experiment III. Duration, 32 days......... | 9 leaves, with center cut out | 10.522 | 22 | 2.292 | 218 |
| | 9 sister leaves, intact | 17.852 | 30 | 3.430 | 192 |

A similar law seems to hold for the root formation though this will have to be determined more definitely. The same law seems also to hold for other cases of regeneration of *Bryophyllum* not discussed in this note.

We can, therefore, state that the quantity of regeneration in an isolated piece of an organism is under equal conditions and in equal time directly proportional to the mass of growth material circulating in the sap (or blood) of the piece and required for the synthetical processes giving rise to the regenerated tissues and organs. If we measure the rate of regeneration by the mass of material regenerated in a given time, the law expressed for the quantity holds also for the *rate* of regeneration and in this form the law becomes a special case of the law of chemical mass action.

4. This law does not throw any light upon two other features of regeneration, namely, first, why it is that as a rule only the apical bud of an isolated piece of stem grows out and none of the buds situated more basally in the stem; and second, why it is that the same bud which grows out when the piece of stem is cut out from the whole plant does not grow out as long as the piece is part of a whole (and normal) plant. The writer published not long ago a series of experiments[2] which suggest that the growing apex (as well as the leaves) of a plant continually produce and send toward the base of the plant substances which *inhibit* the growth of dormant buds. When a piece of stem is cut out from a plant these inhibitory substances contained in the stem will continue to flow toward the base, with the result that the most apical buds will be the first to become comparatively free from these inhibitory substances and hence will be the first to grow out. As soon as this happens, the growing buds will produce and send toward the base inhibitory substances, with the result that none of the more basally situated buds of the piece of stem can grow out. In the normal plant the material serviceable for growth can only be utilized by the growing region at the apex (and the base of the plant); and when a piece is cut out from the plant the same material becomes available for the growth of those buds which are the first to be freed from the inhibitory material which they contained while forming parts of the whole plant. The further qualitative as well as quantitative experiments which the writer has carried out since the publication of his preliminary note support this hypothesis.

TABLE 3

INFLUENCE OF MASS OF LEAVES UPON MASS OF SHOOTS REGENERATED BY STEM

| | | WEIGHT OF LEAVES | WEIGHT OF 6 REGENERATED SHOOTS ON STEM | MILLIGRAMS OF SHOOTS REGENERATED PER GRAM OF LEAF |
|---|---|---|---|---|
| | | *grams* | *grams* | |
| Experiment I. Duration, 37 days.......... | 6 whole leaves | 19.030 | 2.808 | 147 |
| | 6 sister leaves, reduced in size | 2.853 | 0.443 | 152 |
| Experiment II. Duration, 34 days.......... | 6 whole leaves | 18.490 | 3.586 | 192 |
| | 6 sister leaves, reduced in size | 3.503 | 0.668 | 190 |

*Summary.*—By measuring the influence of the mass of a leaf attached to an isolated piece of stem upon the process of regeneration in the piece, it has been possible to prove that the quantity of regeneration is in equal time and under equal conditions in direct proportion to the mass of the leaf. Since nothing except substances produced and sent out by the leaf can vary in direct proportion to its mass, it follows that the quantity of regeneration in an isolated piece of an organism is under equal conditions determined by the mass of material necessary for growth circulating in the sap (or blood) of the piece. If we measure the rate of regeneration by the mass of material regenerated in a given time, the law of regeneration becomes a special case of the law of chemical mass action. That this mass action on a bud is only possible in a piece of stem after it is isolated, the writer explains on the assumption that the apex of an intact plant sends constantly inhibitory substances into the stem preventing the buds contained in the stem from growing and consuming the material required for growth. When a piece of stem is isolated, the supply of these inhibitory substances from the growing region ceases and the most apical bud being the first to become free from the inhibitory substance will then come under the influence of the acting masses of the substances in the sap and regeneration will occur. The mystifying phenomenon of an isolated piece restoring its lost organs thus turns out to be the result of two plain chemical factors: the law of mass action and the production and giving off of inhibitory substances in the growing regions of the organism.

[1] Loeb, J., *Science, New York*, **45**, 1917, (436); *Bot. Gaz., Chicago*, **65**, 1918, (150).

[2] Loeb, J., *Science, New York*, **46**, 1917, (547).

## *NATIONAL RESEARCH COUNCIL*

### MINUTES OF EXECUTIVE BOARD OF WAR ORGANIZATION

First Meeting, Tuesday, February 26, 1918

The meeting convened at 9.30 a.m. in the offices of the Council, 1023 16th Street, Washington, D. C., with Mr. Hale, the Chairman of the Council, in the chair.

Present: Marston T. Bogert, John J. Carty, Whitman Cross, Gano Dunn, George E. Hale, John Johnston, Vernon L. Kellogg, Charles E. Mendenhall, Richard M. Pearce, Charles D. Walcott, Robert S. Woodward, Robert M. Yerkes, and by invitation, Henry N. Russell.

The minutes of the joint meeting of the Council of the National Academy of Sciences and the Executive Committee of the National Research Council of January 17, 1918, were presented and approved.

The Chairman of the Council reported:

1. That arrangements have been made to rent the building at 1015 16th Street N.W., at a rental of $250 per month, from March 1 to September 15, 1918, in order to accommodate the growing activities of the Council, particularly in coöperation with the U. S. Signal Corps.
2. That Mr. Johnston has accepted his election as Executive Secretary of the Council to date from February 1, 1918.
3. That Sidney W. Farnsworth of the Westinghouse Electric Company has been appointed Technical Assistant of the London Section of the Research Information Committee, and that all members of the London and Paris Sections of this Committee have sailed for their posts of duty, with the exception of Mr. Farnsworth, who will leave shortly.
4. That for personal reasons it has become necessary for Harold D. Babcock, Technical Assistant of the Washington Section of the Research Information Committee, to resign and to return to California. His resignation took effect on February 17, 1918.
5. That Graham Edgar of Throop College of Technology, has accepted appointment as Technical Assistant of the Washington Committee in Mr. Babcock's place and expects to assume his duties about March 15.
6. That Clarence H. Mackay has contributed the sum of $1000 for expenses in connection with the work of the Sub-Committee on Protective Body Armor of the Engineering Committee of the Council.
7. That the Chairman has prepared a report of the activities of the Council for the year 1917, to be incorporated in the annual report of the National Academy of Sciences, and that with this report will also appear a statement of the contributions and appropriations which have been made available for expenditure under the direction of the Council or with its coöperation.

The Chairman of the Council submitted the following report of action taken by the Executive Committee of the Executive Board of the Council under authority granted at the joint meeting of the Council of the National Academy of Sciences and the Executive Committee of the National Research Council, on January 17.

1. That the following six members at large have been appointed to membership on the Executive Board of the Council:

John J. Carty, Arthur A. Noyes, Van H. Manning, Michael I. Pupin, William H. Welch and Robert S. Woodward.

2. That it has been impossible for Major Flexner to accept the chairmanship of the Division of Medicine, Hygiene, Surgery, Anatomy, Anthropology, Physiology, Psychology and Zoology, and that Richard M. Pearce, of the University of Pennsylvania, has been appointed Chairman in his place, and that the title of the Division has been changed to read "Division of Medicine and Related Sciences."

3. That an Executive Committee of the Division of Medicine and Related Sciences has been appointed as follows:

Richard M. Pearce, Chairman, Robert M. Yerkes, Vice-Chairman, Charles B. Davenport, Simon Flexner, William H. Howell, Charles H. Mayo, William J. Mayo, F. F. Russell, Edward R. Stitt, V. C. Vaughan, and William H. Welch.

4. That an Executive Committee of the Division of Geology and Geography has been appointed as follows:

John C. Merriam, Chairman, Whitman Cross, Vice-Chairman, Frank W. DeWolf, Douglas W. Johnson, and Philip S. Smith.

5. That Vernon L. Kellogg, of Leland Stanford Junior University, has been appointed Chairman of the Division of Agriculture, Forestry, Botany, Fisheries and Zoology.

6. That Charles E. Mendenhall has been appointed Vice-Chairman of the Division of Physics, Mathematics, Astronomy and Geophysics.

7. That Henry M. Howe, Bedford Hills, New York, has accepted appointment as Chairman of the Section on Metallurgy of the Engineering Division of the Council.

The Chairman also presented the following financial statement showing expenditures on account of the work of the Council since the meeting of the Executive Committee of January 17.

*Account in Riggs National Bank*

| | | |
|---|---|---|
| Balance on hand January 17, 1918 | | $551.93 |
| Total deposits January 17 to February 26 | | 5000.00 |
| | | $5551.93 |
| Salaries | $547.59 | |
| Expenses and Supplies | 3615.43 | |
| Equipment | 568.62 | |
| Total expenditures | 4731.64 | 4731.64 |
| Balance on hand | | $820.29 |

Upon motion Colonel Carty was nominated and elected as Chairman of the Executive Board of the Council, and thereupon occupied the chair.

The resignation of Russell H. Chittenden as Chairman of the Committee on Research in Educational Institutions was presented and accepted with regret.

After discussion the appointment of an Executive Committee of the Executive Board of the Council was reconsidered and upon motion the previous action in establishing such a Committee was rescinded. Thereupon it was voted, upon motion of Mr. Dunn, that a small Committee with executive powers, consisting of the general officers of the Council and of the Chairmen of its Divisions, be appointed, to be known as the Interim Committee, to serve between meetings of the Executive Board for consideration of current business. It was also voted that five members of the Interim Committee shall constitute a quorum.

Mr. Dunn spoke of the desirability of maintaining reciprocal relations and associations with the Engineering Foundation, and after discussion it was

RESOLVED, that in conformity with the policy of The Engineering Foundation expressed in its resolution of September 20, 1917, addressed to the National Research Council, and subject to the approval of The Engineering Foundation, the National Research Council hereby designates Alfred D. Flinn, secretary of The Engineering Foundation, as assistant secretary of the National Research Council, without salary, for a period of one year from date, and in turn is ready reciprocally to approve the designation by The Engineering Foundation of John Johnston, secretary of the National Research Council, as an assistant secretary of The Engineering Foundation on the same terms and for the same period.

Upon suggestion of the Chairman of the Council, an Information Section of the Administrative Division was established, to include the work of the Research Information Committee and other similar activities. He read the following statement relative to the purpose and functions of the Research Information Committee.

### RESEARCH INFORMATION COMMITTEE

1. By joint action the Secretaries of War and Navy, with the approval of the Council of National Defense, have authorized and approved the organization, through the National Research Council, of a Research Information Committee in Washington with Branch Committees in Paris and London, which are intended to work in close coöperation with the offices of the Military and Naval Intelligence, and whose function shall be the securing, classifying, and disseminating of scientific, technical, and industrial research information, especially relating to war problems, and the interchange of such information between the Allies in Europe and the United States.

2. In Washington the Committee consists of:

(a) A civilian member, representing the National Research Council, S. W. Stratton, Chairman.

(b) The Chief, Military Intelligence Section.

(c) The Director of Naval Intelligence.

3. The initial organization of the Committee in London is:

(a) The Scientific Attaché, representing the Research Information Committee, H. A. Bumstead, Attaché.

(b) The Military Attaché, or an officer deputed to act for him.

(c) The Naval Attaché, or an officer deputed to act for him.

4. The initial organization of the Committee in Paris is:

(a) The Scientific Attaché, representing the Research Information Committee, W. F. Durand, Attaché.

(b) The Military Attaché, or an officer deputed to act for him.

(c) The Naval Attaché, or an officer deputed to act for him.

5. The chief functions of the foreign committees thus organized are intended to be as follows:

(a) The development of contact with all important research laboratories or agencies, governmental or private; the compilation of problems and subjects under investigation; and the collection and compilation of the results obtained.

(b) The classification, organization, and preparation of such information for transmission to the Research Information Committee in Washington.

(c) The maintenance of continuous contact with the work of the offices of Military and Naval Attachés in order that all duplication of work or crossing of effort may be avoided, with the consequent waste of time and energy and the confusion resulting from crossed or duplicated effort.

(d) To serve as an immediate auxiliary to the offices of the Military and Naval Attachés in the collection, analysis, and compilation of scientific, technical, and industrial research information.

(e) To serve as an agency at the immediate service of the Commander-in-Chief of the Military or Naval Forces in Europe for the collection and analysis of scientific and technical research information, and as an auxiliary to such direct military and naval agencies as may be in use for the purpose.

(f) To serve as centers of distribution to the American expeditionary forces in France and to the American naval forces in European waters of scientific and technical research information, originating in the United States and transmitted through the Research Information Committee in Washington.

(g) To serve as centers of distribution to our Allies in Europe of scientific, technical, and industrial research information originating in the United States and transmitted through the Research Information Committee in Washington.

(h) The maintenance of the necessary contact between the offices in Paris and London in order that provision may be made for the direct and prompt interchange of important scientific and technical information.

(i) To aid research workers, or collectors of scientific, technical, and industrial information from the United States, when properly accredited from the Research Information Committee in Washington, in best achieving their several and particular purposes.

6. The headquarters of the Research Information Committee in Washington is in the offices of the National Research Council, 1023 Sixteenth Street; the Branch Committees are located at the American Embassies in London and Paris.

It was decided upon nomination of the Chairman of the Council to request the President of the National Academy of Sciences to appoint the following gentlemen, who will in future have an active part in its work, as additional members of the Council:

Henry M. Howe, Emeritus Professor of Metallurgy, Columbia University.

Vernon L. Kellogg, Professor of Entomology, Leland Stanford Jr. University.

S. L. G. Knox, President, Knox Engineering Company, San Francisco, California.

William J. Mayo, Surgeon General's Office, U. S. A.

Charles H. Mayo, Rochester, Minnesota.

Richard M. Pearce, Professor of Research Medicine, University of Pennsylvania.

Colonel F. F. Russell, Surgeon General's Office, U. S. A.

Rear Admiral Edward L. Stitt, Medical Director, Naval Medical School.

Robert S. Woodward, President, Carnegie Institution of Washington.

Mr. Johnston reported verbally with regard to plans for the section on Industrial research and Mr. Hale stated that Mr. Elihu Root has accepted appointment as a member of the Advisory Committee of this section.

The Chairman of the Council reported with regard to proposals for the organization of the Engineering Division and also stated that it has been decided to organize a Section on Design in the Science and Research Division of the Signal Corps, under the chairmanship of Mr. S. L. G. Knox.

Upon nomination of the chairman of the Division of Physics, Mathematics, Astronomy, and Geophysics, the appointment of a Camouflage Committee was approved with membership as follows:

M. Luckiesh, Chairman, Bassett Jones, Lloyd A. Jones, G. H. Thayer.

The Chairman of the Division of Chemistry and Chemical Technology reported that Samuel Avery, Chancellor of the University of Nebraska, has come to Washington to aid in the work of the Division, to which he is devoting his entire time. He also reported on the progress of work under this Division and of the close and valuable coöperation which is being maintained with the War Industries Board and other Governmental agencies.

The Chairman of the Division of Geology and Geography explained the activities of this Division and outlined the nature of the special monograph work which has been proposed by the Army War College. After discussion it was voted that the question of allotment of a sum not to exceed $6000, to be expended for this work, be referred to the Interim Committee, with authority to act. Explanation was made that of this amount the sum of $1000 would be needed for the expenses of the Assistant to Major D. W. Johnson, who expects to go to France to obtain information for the monograph work, the remaining $5000 to be used for the expenses of assistants to be engaged upon this work in Washington.

The Chairman of the Division of Medicine and Related Sciences read a report in regard to the organization, purpose, and work of this division as follows:

*Purpose.*—To concentrate in Washington a comparatively small body of men representing the existing committees, and thus provide for effective coöperation in the rapid organization of medical research as an aid to the solution of urgent military problems.

*Field.*—Medicine, Surgery, Hygiene, Physiology, Anatomy, Psychology, Psychiatry, Physical Anthropology, and closely related subjects.

*Methods.*—1. To coöperate closely with the Surgeon General of the Army (through Colonel Russell) and of the Navy (through Dr. Stitt) in determining urgent problems and to enlist the aid of civilian laboratories in the solution of these problems.

2. To assist the Surgeons General of the Army and Navy in procuring trained investigators to enter the respective services as contract surgeons to undertake special field investigations during short periods of time.

3. To send, if it is considered advisable, individuals to England, France, and Italy to determine the urgent problems which should be taken up without loss of time in civilian laboratories in this country.

4. To invite, if it is considered necessary, commissions or individuals from England, France and Italy to this country to advise with the Medical Division of the National Research Council.

5. To maintain correspondence with prominent medical investigators in the American Expeditionary Forces and in civilian laboratories in France, England and Italy and thus obtain reports of the important fields of research, the character of the work in progress and the needs of the workers.

6. To establish relations with and if agreeable to them, to coöperate with research organizations abroad as (a) British Medical Research Committee, (b) the Research Society recently organized in France by medical officers of the American, French and British forces, and (c) the Committee on Medical Research of the American Red Cross in France.

7. To obtain reports of all medical research organizations in this country dealing with war problems and of individuals engaged in the investigation of war problems.

8. (a) Prepare lists of individuals and laboratories equipped and ready to undertake research at short notice.

(b) Prepare lists of individuals who will hold themselves in readiness to move from laboratory to laboratory to work for shorter or longer periods on special or emergency problems or to augment existing laboratory staffs in a group of selected laboratories.

9. To maintain a bureau for the dissemination of up-to-date bibliographies of all forms of medical research bearing on war problems.

10. To hold conferences from time to time in Washington or other central city for discussion of important research problems and methods of attack.

11. To hold military medical meetings from time to time, in the neighborhood of large cantonments, for the discussion of medical problems by military and civilian physicians.

The Chairman of this Division also outlined the character of investigations already undertaken in coöperation with the Surgeon General's office of the Army. The Chairman of the Council stated that requests had been received for information which might require an extension of the proposed activities of the Anthropology Committee, operating under the Division of Medicine and Related Sciences. Upon motion it was voted that the Interim Committee be authorized to make such additional appointments to membership in the Anthropology Committee as may be advisable in order to provide adequately for future work which may be undertaken by this Committee.

Meeting adjourned at 12 o'clock.

John Johnston, *Secretary.*

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VOLUME 4 MAY, 1918 NUMBER 5

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OF THE

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OF THE

UNITED STATES OF AMERICA

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PROCEEDINGS
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NATIONAL ACADEMY OF SCIENCES
Volume 4 MAY 15, 1918 Number 5

National Museum

# SOME SPECTRAL CHARACTERISTICS OF CEPHEID VARIABLES

By W. S. Adams and A. H. Joy

Mt. Wilson Solar Observatory, Carnegie Institution of Washington

Communicated February 25, 1918

In a recent investigation of the absolute magnitudes of 500 stars[1] we have shown that in a large number of cases the intensity of the hydrogen lines is abnormally great in relation to the spectral type as derived from the more general characteristics of the spectrum. The effect is most striking in the case of giant M-type stars such as $\alpha$ Orionis, but it is marked for many K and G-type spectra as well. The suggestion was made that abnormal intensity of the hydrogen lines is a general characteristic of the giant stars of at least some of the spectral types. A discussion of this question with reference to certain of the variable stars of the $\delta$ Cephei type is the object of this communication.

It has been shown by the investigations of Hertzsprung[2] and others that the Cepheid variables are stars of very high intrinsic luminosity, with an average absolute magnitude, as derived from parallactic motion, more than seven magnitudes brighter than the sun. Directly measured parallaxes of five stars yield a value of about five magnitudes. Whichever result is accepted it is evident that these stars are exceedingly luminous and form most interesting material for a study of the question of the intensity of the hydrogen lines.

The spectrum of the Cepheid variables has been studied by many observers, and it is quite impossible to make adequate reference to their results in this place. In general, however, most of the work of Albrecht[3], Duncan[4], Shapley[5], and many others has dealt with variations in the character of the spectrum at maximum and minimum of light. For the purpose we have in mind a direct comparison is instituted between the spectra of the several variables and that of the sun, which is selected as a typical star of type $G_0$. Photographs of the spectra, all of which were made with a slit spectrograph, taken at maximum and minimum of light are also compared directly with one another. Furthermore typical stars having spectra of types $F_0$, $F_5$, and $G_5$ have been selected from among those classified by the Harvard observers, and these spectra have been used for comparison purposes as well as that of the sun. The spectra have been compared by the aid of a Hartmann spectro-comparator.

A summary of the results is given in brief form in the accompanying table. It became clear from the very first comparisons that the hydrogen lines are abnormal in the Cepheid spectra, being in some cases several times as strong as in the sun, although in other respects the spectra do not differ greatly. We have accordingly adopted the plan used in previous investigations of making two determinations of spectral type. The first is based upon the hydrogen lines alone; the second upon the more general features of the spectra, in particular, the intensities of the arc lines of the various elements, and other characteristics which seem to be primarily a function of general spectral type.

| | MAXIMUM OF LIGHT | | MINIMUM | |
|---|---|---|---|---|
| | Hydrogen | General spectrum | Hydrogen | General spectrum |
| TU Cassiopeiae | $F_0$ | $F_9$ | $F_8$ | $F_9$ |
| SU Cassiopeiae | $F_3$ | $F_7$ | $F_6$ | $F_8$ |
| SZ Tauri | $F_0$ | $F_8$ | $F_6$ | $F_8$ |
| T Monocerotis | $F_0$ | $G_0$ | $F_7$ | $G_1$ |
| RT Aurigae | $F_0$ | $F_7$ | $F_8$ | $G_0$ |
| W Geminorum | $A_9$ | $G_0$ | $F_6$ | $G_0$ |
| ζ Geminorum | $F_2$ | $F_9$ | $F_3$ | $G_0$ |
| η Aquilae | $F_3$ (near max) | $F_9$ | $F_8$ | $G_1$ |
| δ Cephei | $F_2$ (near max) | $F_9$ | $F_9$ | $G_0$ |
| Mean | $F_1$ | $F_9$ | $F_7$ | $G_0$ |

Three conclusions may be drawn from these results: First, that the hydrogen lines are abnormally strong in all of these stars, the difference translated into spectral type amounting to eight divisions of the Harvard scale at maximum of light: Second, that at minimum this difference is reduced, amounting in the average to but three divisions: Third, that there is little or no difference in the general spectrum at maximum and minimum so far as the criteria here used are concerned. Certain differences in other respects will be referred to presently. The general conclusion may therefore be drawn that the variation in spectral type among the Cepheid variables is mainly a variation in the intensity of the hydrogen lines.

The determination of spectral type of several Cepheid variables by Shapley[5] and of δ Cephei by Adams and Shapley[6], were based wholly on the hydrogen lines, the anomalous behavior of these lines in many stars not being fully recognized when the earlier work was done. A comparison of the present spectral determinations from the hydrogen lines with those of Shapley for seven stars common to the two lists shows a close degree of accordance in the amount of variation at maximum and minimum of light. The values are seven and eight spectral divisions in the two cases respectively. There is, however, a small systematic difference in spectral type at both maximum and minimum, the types given here being on the average slightly earlier.

It is clear from the very special character of the variations in the spectra of the Cepheid variables that it is doubtful if they can be regarded as furnishing direct evidence as to the order of evolution of stars in general. In the normal succession of spectral types changes in intensity of the hydrogen lines are accompanied by numerous important changes in many features of the spectrum which appear to remain essentially unaltered in the spectra of the Cepheids.

Reference has been made to certain differences in the spectra of these stars at maximum and minimum of light other than those in the hydrogen lines. The most important of these are: (1) a shift of the maximum of the continuous spectrum toward shorter wave-lengths at maximum of light, a result found by Albrecht[3]: (2) a general slight widening of the spectral lines at minimum: (3) an increase in the intensity of the so-called 'enhanced' lines at maximum. The last two characteristics were noted in the investigation of the spectrum of δ Cephei by Adams and Shapley[6]. The change in the enhanced lines is probably most significant in its bearing on the variation of absolute magnitude. The three lines principally used for the determination of absolute magnitudes of stars of this type of spectrum are all strongly enhanced and it seems probable, as we have suggested previously, that enhanced lines as a class vary with luminosity. Accordingly we have compared the intensities of some of the more prominent enhanced lines in the spectra of these variables with their intensities in the solar spectrum, and also on the photographs taken at maximum and minimum of light. In all cases these lines are much more intense in the stellar spectra, and to a less degree more intense at maximum than at minimum of light. Among the lines are the following:

4077 Sr
4215 Sr
4233 Fe
4246 Y
4290 Ti
4376 —
4385 Fe
4534 Ti
4584 Fe

Of these λ 4077 shows the largest difference, being fully five times as strong in some of the stellar spectra as in the sun. With the aid of the reduction curves for absolute magnitude, the difference in the intensities of the three lines λ 4077, 4215 and 4290 at maximum and minimum of light may be converted into differences of magnitude. They give for the average of the nine stars the values 0.8, 1.3 and 0.6, respectively, or a mean of 0.9. This is in very fair agreement with the average variation in apparent magnitude of 0.7 for the same stars.

Two other features of the spectra of this class of variables may be referred to briefly. The first is the marked difference in the relative intensities of the hydrogen lines when compared with the solar spectrum. In the sun the line Hβ is considerably more intense than Hγ, but in all of the Cepheid spectra the reverse is the case, Hγ being much stronger than Hβ. At minimum of light this effect is slightly less marked than at maximum. It seems probable that we are dealing here with a shift in the maximum of intensity of the line spectrum of hydrogen similar to that which occurs in the continuous spectrum. It has been shown in the laboratory that in the case of lines belonging to the same series increase of temperature increases the intensity of the more refrangible lines, the maximum moving toward shorter wave-lengths. In such high luminosity stars as the Cepheid variables such a difference as compared with the sun is highly probable.

It is of interest to note in connection with a study of the spectra of these variable stars that the spectra of certain stars which, with the exception of $\rho$ Cassiopeiae, are not known to show light variation have very similar characteristics both as regards the intensity of the hydrogen and the enhanced lines. The most promient cases of this kind are the following:

| | R. A. | DEC. | GAL. LONG. | GAL. LAT. |
|---|---|---|---|---|
| $\delta$ Canis Majoris | $7^h$ $4^m$ | −26° 14′ | 204° | −8° |
| $\xi$ Puppis | 7 45 | −24 37 | 211 | 0 |
| $\gamma$ Cygni | 20 19 | +39 56 | 45 | +2 |
| Boss 5931 | 22 56 | +56 25 | 75 | −5 |
| $\rho$ Cassiopeiae | 23 49 | +56 57 | 83 | −5 |

The star $\delta$ Canis Majoris has been found at the Lick Observatory to show a small variation in radial velocity. Some of these stars have spectra nearly identical with that of the Cepheid variables, and it is an interesting fact that all of them are situated very near the galactic plane and thus share in the peculiar distribution of the latter.

---

[1] Adams, W. S., and Joy, A. H., *Mt. Wilson Contr.* No. 142; *Astroph. J., Chicago,* **46**, 1917, (313–339).

[2] Hertzsprung, E., *Astr. Nachr., Kiel,* **196**, 1913, (201–208).

[3] Albrecht, S., *Lick Obs. Bull., Berkeley,* No. 118.

[4] Duncan, J. C., *Ibid.*, No. 151.

[5] Shapley, H., these PROCEEDINGS, **2**, 1916, (208–209); also *Mt. Wilson Contr.* No. 124.

[6] Adams, W. S., and Shapley, H., these PROCEEDINGS, **2**, 1916, (136–142).

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# TYPES OF ACHROMATIC FRINGES

By CARL BARUS

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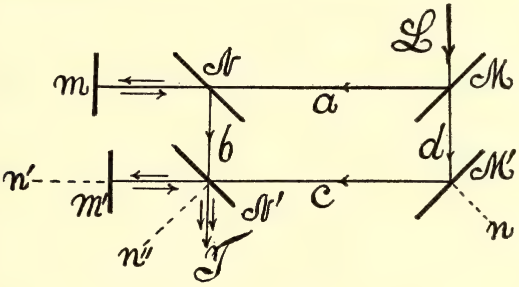
Communicated, March 18, 1918

The difficulty of obtaining fringes of the strictly achromatic type (i.e., two strong fringes with a black line between and the remaining fringes greenreddish and faint) in the rectangular or other interferometer, has been referred to in my earlier papers. As a rule the fringes found are more or less diffuse, non-symmetric, with large numbers (10 or more) about equally strong. Such fringes are, of course, useless in displacement interferometry. When the sharp fringes needed are obtained, their definition is independent of the particular part of any of the glass plates used, and any plate may be rotated 180° in its own plane without spoiling the sharpness of the fringes. Hence such slight curvatures or wedge shapes as the plates may possess, are without influence on the phenomenon. To further test this I devised a screw press adapted to push the vertical edges of a plate to the rear and the middle forward, so as to give the plate marked cylindricity. Quarter and eighth inch plates were operated on, in the latter case sufficiently to give the two superposed slit images quite unequal width; but no essential or useful improvement of the fringes was observed. The type of the fringe was not altered. Again the symmetrical fringes may be obtained from plates thickly or thinly silvered, without essential difference.



It follows therefore, that the relative thickness of the glass paths traversed by the interfering beams can alone be of influence in shaping the fringe pattern in the manner in question. This is in consonance with the general theory of achromatic fringes, the result being a superposition of the color phenomena due to the dispersive refraction of the glass and the colors resulting from the wave lengths of the interfering rays. To test this the annexed form of apparatus (see figure) is particularly convenient, as the fringes are easily found. Moreover both rays, $a$ and $c$, from the collimator at $L$, eventually pass through the plate, $N'$, before reaching the telescope at $T$. It is thus merely the thickness of the half silvers, $M$ and $N$, both at 45°, that is here in question. If this thickness is the same, the sharp symmetrical design of but two strong fringes appears. If the difference of thickness is but little over half a millimeter, many fringes, non-symmetric in distribution are the rule. If the differential thickness is several millimeters there may be hundreds of fringes. If these are small they may be enlarged at pleasure; but they are always faint and useless for measurement.

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[1] Advance note from a Report to the Carnegie Institution of Washington.

# *INTERFERENCE OF PENCILS WHICH CONSTITUTE THE REMOTE DIVERGENCES FROM A SLIT*

By Carl Barus

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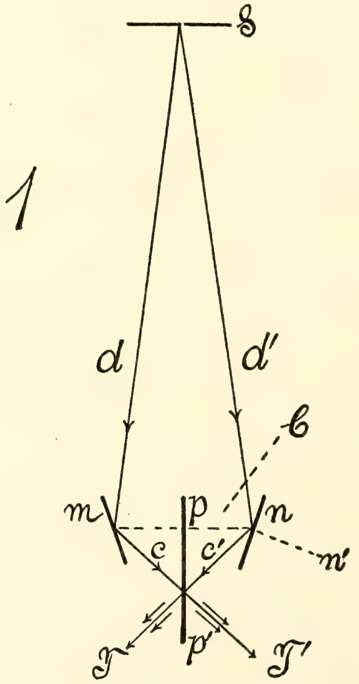
Communicated, March 18, 1918

1. The sharp prism heretofore[2] used for cleaving the rays issuing from a collimator (or the slit simply) was dispensed with and the endeavor made to obtain two rays capable of interference. The assemblage of apparatus is shown in figure 1, where $S$ is the slit (to be replaced by a Nernst or a tungsten filament), $m$ and $n$ the opaque mirrors, $pp'$ the half silvered plate. The rays $dd'$, from $S$, pass after reflection into $c$ and $c'$ and may be observed by spectrotelescopes placed either at $T$ or $T'$. In the first experiments the distance $dp'$ was about 4 meters and the distance $mn$, 10 cm. The mirrors $n$ and $pp'$ were on micrometers with the screws normal to their respective faces. The distance $mn$ must be within the limits of the wedge of light from the slit and is therefore small, unless $d$ is very large. Both $pp'$ and $n$ are on the



rotating rail, whereas *m* is fixed. The apparatus was also adjustable for reversed rays by attaching an auxiliary mirror, normal to the rays $d'$ prolonged through *n*. As *S* is distant, this slit must be long as otherwise the spectrum band will be a mere horizontal line and the fringes difficult to detect. A doublet of lenses each about 10 cm. in diameter, of the same focal power (1/60 cm.) but respectively convex and concave and having a combined focal distance of about 5 or 6 meters, is of advantage for focussing a large solar image (1 to 2 inches in diameter) on the slit. The Nernst or tungsten filament gives the same advantages at once, but the former is too thick.

The fringes are exceedingly difficult to find in spite of the brilliant spectra. It was not until after about three days of searching, in which besides sunlight I used the filaments as well as the methods of direct and of reversed rays that I ultimately succeeded with the former. The fringes lie quite sharply in a definite focal plane, usually between that of the slit image and the principal focal plane. After being found they are strong elliptic spectrum fringes; but when lost nevertheless difficult to rediscover.

The achromatics which coincide in adjustment with horizontal spectrum fringes and are seen with the slit image out of focus, are additionally difficult to find because of the short height of the slit image. As first obtained they lacked brilliancy and were not easily observed.

An attempt was made to register slight lateral displacements of the slit in terms of the displacement of fringes; but as the slit images are thrown out of coincidence when the slit moves, trustworthy numerical data could not be obtained. Incidentally it might appear that two vertical lines of the slit 0.0014 cm. apart should wipe out each other's fringes as a case of interference; but this is not the fact as slit widths over 100 times broader are admissible.

2. After completing these experiments, the distance between slit *S* and the mirrors *mn* was increased to about nine meters. The same lens doublet focussing a large solar image on the slit was used as before. When the fringes were found, in view of the longer distance, *d*, the slit could be opened to over a millimeter of breadth before they quite vanished, from the spectrum.

Operating with two successive slits at about 9 meters from the interferometer, one of which received the light through the other, I found that two independent sets of fringes very different in size and inclination could be put in the field together. Further investigation showed that the size and inclination of the fringes is essentially dependent on the degree of parallelism of the two corresponding slit images. When the images are parallel, the fringes are of maximum size and vertical. When the images are not quite parallel (they incline in opposite directions when the slit is slightly rotated in its own plane from the vertical), the fringes rapidly grow smaller, rotate and vanish. With parallel slit images the spectrum ellipses are centered in the field; otherwise they are very far out of center. The adjustment for actual (not x-like) coincidence must therefore be made with precision, if the fringes are to appear.

Further work was also done with sunlight to obtain more pronounced achromatics. For this purpose a compensator, *C*, was inserted to equalize the glass path in the half silvered plate. Huge spectrum ellipses were obtained in this way and their centers were placed above the telescopic field so that the fringes seen were large horizontal bars. On removing the spectroscope and placing the slit images out of focus, brilliant achromatics were now obtained, of the concentric hyperbolic type, vividly colored and broad between the apices, and diminishing to hair lines laterally. With these it was possible to enlarge the slit to at least 3 mm., without destroying the fringes, though they became more vague. It is again necessary that the slit images, when in focus, should be quite parallel. It was possible to place a plate of ground glass on the far side of the slit, without destroying the fringes; but not on the side towards the interferometer. In other respects the behavior was as described in the case of achromatics in the earlier experiments with a cleavage prism.

Finally the spectrum fringes and the corresponding achromatics were obtained with the light of a Nernst filament, by focussing an image of it with a strong condenser lens on the slit. The experiments however are very difficult. The spectrum fringes are often weak, out of focus and extremely sensitive to small disadjustments in the horizontal and vertical coincidence of the slit images. They require a fine slit. When well produced the achromatics are also obtainable on removing the spectroscope when the spectrum fringes are horizontal bars. The achromatics may also be obtained brilliantly without the condenser lens; but the adjustment must in such a case be made first with sunlight, as the spectrum from the Nernst filament is too feeble for detecting fringes so elusive as the present. The achromatics however are strong and brilliant even here (Nernst filament).

An interesting result is obtained in case of the achromatic fringes by narrowing one of the beams, for instance that coming from the mirror *m* (figure 1), by a screen with a slit about 2 mm. wide. In such a case the slit image, out of focus, is correspondingly narrowed. It may be passed from side to side of the broad washed slit image coming from the mirror *n*, by moving its adjustment screws (vertical axis). The fringes then appear only in a particular position of the narrow image in the field of the broader; but when they do appear they spread far beyond the margins of the narrow image on both sides. Interference thus apparently occurs where but one beam is present. The phenomenon is like those instanced before and means, as I understand it, that the beams have met in some other focal plane, though one is tempted to conclude that interference is stimulated by resonance, in particular as it is often impossible to find a plane in which they have met. The achromatics may sometimes be seen before and behind the principal focal plane, but more frequently either in the one or in the other region, only.

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[1] Advance note from a Report to the Carnegie Institution of Washington.

[2] These PROCEEDINGS, 3, 1917 (565).

# A STUDY OF THE MOTIONS OF FORTY-EIGHT DOUBLE STARS

By Eric Doolittle

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The double stars examined in the following investigation will all be recognized as old and well-known pairs, on each of which a large number of measures have accumulated. Each of these has shown a considerable motion since discovery, but although orbits have been computed for several of them, there is no case in which certain knowledge of the true form of the orbit can be had at this time, nor with the greater number of these pairs is it even certain that the motion is orbital at all. It was, in fact, in view of this uncertainty that the following computations were undertaken. The complete list of double stars at present known to us was examined, and all of those in which, notwithstanding the large number of measures made upon them, it seemed uncertain whether the motion is rectilinear or orbital were selected for study. The primary object was to ascertain, when possible, in which pairs rapid motion during the next few years might be reasonably expected and those in which the distance is now near a maximum value. It is upon such pairs that observations are most urgently needed, while upon those for which the analysis gives no indication that the companion is now near a critical part of the orbit the multiplication of observations is at the present time unnecessary.

It is well known that with the type of pairs here considered, in which less than two quadrants have been described by the companion, a series of widely different orbits may be obtained, any one of which may satisfy the observations within the limits of error naturally to be expected; and this, even although it may afterward appear that the motion is only rectilinear and the pair is not a true binary system. It was therefore thought best not to begin each computation by so adjusting the six constants of the orbit that the observations should be represented by them, but rather to assume that the motion is rectilinear, and having found the straight line which best represents all the observations to examine in each case whether there is definite evidence that the motion of the companion in the straight line is, or is not a uniform motion. If a continuous progressive change is found, greater than can be assumed due to errors of observation, it is evident that the motion is not rectilinear, while in the many cases in which the variations are less, or but little greater, than might be attributed to errors of observation the evidence for the binary character of the system must be regarded as inconclusive.

The first step in each case was to form suitable means of the numerous observations. Among the earlier measures, those of Maedler and also those of the few occasional observers were rejected, especially since in no case were there wanting accurate observations by Σ, 0Σ and Δ during the same intervals. Practically all modern observations were, however, included, and these were weighted in proportion to the number of nights, except that at least double weight was always given to the measures of Aitken and Hussey with the 36-inch and to those of Burnham with the 40-inch.

The approximate rectilinear path having been found, the differential corrections $\Delta v$, $\Delta T$ and $\Delta d$ to the mean motion, the time of closest approach and the direction, respectively, were next found by a least square adjustment, measures of angle, only, being employed; the distance at closest approach was next found from the weighted means of the measured angles and distances. It was assumed that the line as thus finally adjusted was the best determination of the path obtainable upon the hypothesis that the path is rectilinear. The average velocity, $v$, along the path was finally compared with the observed velocity at different epochs in order to ascertain any systematic variation that might exist.

It is found convenient to group the pairs examined into five classes, determined by the results of the investigation as follows:

*Class A.*—The velocity shows a decided and certain increase. Rapid motion in angle is to be expected, and careful measures, particularly of angle, are especially needed at this time.

0Σ 20, (Burnham's General Catalogue, No. **479**). Though for 50 years after discovery the velocity remained nearly constant, (about 0″.016), it has, since 1897, very decidedly increased, (to 0″.036), and orbital motion is certain. The period will, however, much exceed the value, 136.2 years, assigned by Glasenapp.

Σ 208, (**1074**). It was found quite impossible to represent both the earlier and the later observations by a single rectilinear path. The velocity is now nearly three times the average velocity and the stars are near their minimum distance.

Σ 963, (**3625**). The motion is here very slow, but there is a systematic increase in $v$, from 0″.0070 in 1839 to 0″.0131 in 1908.

Σ 1306, (**4923**). The observations from 1845–'65 are discordant but there is a remarkable increase in $v$ during recent years. Undoubtedly binary, and now so near the least distance that an entry into the first quadrant may be expected during the next few years. The brighter component (5.0 magnitude) is one of Newcomb's Fundamental Stars and is contained in the American Ephemeris. As we, of course, know nothing of the location of the center of gravity between this and its 8.5 magnitude companion, it would seem that this is unsuitable for use as a fundamental star.

Σ 1834, (**6832**). The distance in this pair is now but little greater than 0″.1 and the measures since 1892 are hopelessly inconsistent. Analysis of the motion from discovery until the date mentioned shows a steady, and finally rapid, increase in velocity. The angle of the companion has probably increased 180° during the past few years.

Σ 113, (**707**) and Σ 2576, (**9602**), also show a definite and certain increase of velocity in recent years.

*Class B.*—The velocity shows a definite diminution. The stars are nearly at their maximum distance. Measures are now needed to fix the position of apastron, but the angle will probably change but little. The distance should be measured with special care.

Σ 305, (1427); Σ 1457, (5508); 0Σ 261, (6415); and 0Σ 288, (7049) all show a decided and progressive diminution of the velocity. In the last, (0Σ 288), it is, in fact, uncertain whether the motion did not wholly cease about 1912, and the two stars begin to draw together.

*Class C.*—A systematic and certain variation of the velocity, but the companion is not certainly at either periastron or at apastron.

The most striking examples in this class are: Σ 400, (1747), in which the velocity has been steadily increasing since discovery; 0Σ 215, (5365), in which the analysis shows the maximum velocity to have been attained about 1893; Σ 1865, (6955), in which the velocity mounted quite suddenly to a value nearly four times the average about 1897, when the stars were closest together, and which has since been rapidly diminishing, and Σ 2118, (7834), in which the maximum velocity was attained about 1895. In all of these cases the rise and fall of the velocity is very striking. There were also found to belong to this class,—Σ 2, (21); Σ 535, (2161); Σ 1536, (5765); and Σ 1687, (6296).

*Class D.*—The velocity apparently varies, (sometimes quite irregularly), and orbital motion is strongly suggested, but it is in no case entirely certain. But few observations are required on these pairs at this time.

Fourteen of the 48 pairs were found to belong to this class in which, though orbital motion might be strongly suspected, it is, nevertheless, not yet entirely certain. These pairs are,—

Σ 13, ( 92),
Σ 138, ( 830),
0Σ 50, (1568),
Σ 460, (1952),
0Σ 159, (3678),
Σ 1175, (4402),
Σ 1338, (5030),
Σ 1429, (5421),
Σ 1517, (5707),
Σ 1757, (6530),
Σ 2315, (8548),
Σ 2434, (8986),
0Σ 413, (10533),
0Σ 437, (10922).

*Class E.*—No certain trace of orbital motion. A multiplication of observations on these pairs is unnecessary.

Fifteen pairs showed no certain trace of orbital motion:

*H* 1968, ( 216),
0Σ 18, ( 374),
0Σ 43, (1365),
Σ 367, (1623),
Σ 567, (2272),
0Σ 92, (2445),
*H* 3823, (3112),
Σ 1104, (4098),
Σ 1423, (5385),
0Σ 237, (5859),
Σ 1643, (6174),
Σ 1883, (7013),
0Σ 297, (7320),
Σ 2199, (8118),
Σ 2574, (9570).

*Summary.*—The final summary for the 48 pairs examined is as follows:
Class A....7 Pairs, Class B....4 Pairs, Class C....8 Pairs.
Total pairs in which orbital motion is certain..........................19
Class D....14 Pairs, Class E....15 Pairs
Total pairs in which the evidence thus far is negative................29

# THE STRUCTURE OF AN ELECTROMAGNETIC FIELD

By H. Bateman

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Communicated by A. A. Noyes, March 18, 1918

1. Starting with the fundamental equations

$$c \operatorname{rot} \mathbf{H} = \frac{\partial \mathbf{E}}{\partial t} + \rho \mathbf{v}, \qquad \operatorname{div} \mathbf{E} = \rho,$$

$$c \operatorname{rot} \mathbf{E} = -\frac{\partial \mathbf{H}}{\partial t}, \qquad \operatorname{div} \mathbf{H} = 0,$$

we adopt as an elementary solution

$$\mathbf{H} = \operatorname{rot} \mathbf{A},\ \mathbf{E} = -\frac{1}{c}\frac{\partial \mathbf{A}}{\partial t} - \nabla\Phi,\ \Psi = \operatorname{div} \mathbf{A} + \frac{1}{c}\frac{\partial \Phi}{\partial t},\ \rho = -\frac{1}{c}\frac{\partial \Psi}{\partial t},\ \rho\mathbf{v} = c\nabla\Psi,$$

where

$$\mathbf{A} = c\int_{-\infty}^{\alpha} f(\tau)\, \nabla \log\, [\mathbf{s}.\mathbf{r} - c\,(t - \tau)]\, d\tau,$$

$$\Phi = -\int_{-\infty}^{\alpha} f(\tau) \frac{\partial}{\partial t} \log\, [\mathbf{s}.\mathbf{r} - c\,(t - \tau)]\, d\tau.$$

In these expressions $\mathbf{s}$ is a unit vector depending on $\tau$, $\mathbf{r}$ denotes the radius vector from the point with co-ordinates $\xi(\tau)$, $\eta(\tau)$ $\zeta(\tau)$, to the point with co-ordinates $x$, $y$, $z$, while $\alpha$ is defined by the equation

$$[x - \xi(\alpha)]^2 + [y - \eta(\alpha)]^2 + [x - \zeta(\alpha)]^2 = c^2(t - \alpha)^2;\ \alpha \leqq t.$$

The function $\Psi$ is given by the formula

$$\Psi = \frac{c}{\nu} f(\alpha),$$

where

$$\nu = \xi'(\alpha)\,[x - \xi(\alpha)] + \eta'(\alpha)\,[y - \eta(\alpha)] + \zeta'(\alpha)\,[z - \zeta(\alpha)] - c^2(t - \alpha).$$

This elementary field corresponds to a state of affairs in which electric charges of a concentrated form are created and travel along straight lines with the velocity of light, the directions of these lines being specified by the different values of the unit vector **s**. Whenever a concentrated electric charge is created an amount of electricity which will just compensate it is fired out in all directions and provides an elementary 'aether' which is the seat of the electromagnetic field of the concentrated charge. A concentrated electric charge and its elementary aether lie at any instant on a sphere whose centre is at the point where these charges originated[1]; if now this point moves with a velocity less than the velocity of light the different spheres bearing electricity that exist at time $t$ do not intersect and if the arbitrary function $f(\alpha)$ is never zero there will be a sphere through each point of space so that our elementary aethers will fill the whole of space; if however the function $f(\alpha)$ is sometimes zero, for axemple if it is zero when $\alpha$ is less than $\alpha_0$, then the elementary aethers will not fill the whole of space.

If we subtract from the above field another one of the same type in which the unit vector function **s** has a different value we obtain a field in which $\rho\mathbf{v}$ and $\rho$ are zero except in the neighbourhood of the concentrated electric charges, there is thus a cancelling of electricity when the two elementary aethers are superposed and we get an aether in the ordinary sense of the word. The field is now one in which concentrated charges of opposite signs are continually produced by a process of separation analogous to that described by Heaviside in 1901. The field thus obtained belongs to the type in which there is a rectilinear flow of energy and no accumulation of energy at any point of space: the energy in such a field may therefore be regarded as kinetic energy or energy of motion.

The most general field that possesses the property just mentioned and the additional property that the volume charge and current in the aether are zero outside the singularities of the field is obtained by writing

$$\mathbf{M} = \mathbf{H} + i\mathbf{E} = cF(\alpha,\beta)\{\nabla\alpha \times \nabla\beta\} = iF(\alpha,\beta)\left\{\frac{\partial\beta}{\partial t}\nabla\alpha - \frac{\partial\alpha}{\partial t}\nabla\beta\right\},$$

where $\alpha$ and $\beta$ are defined by equations of type

$$z - ct = f(\alpha,\beta) + (x + iy)\,\theta(\alpha,\beta), \quad z + ct = g(\alpha,\beta) - \frac{x - iy}{\theta(\alpha,\beta)},$$

$f$, $g$ and $\theta$ being arbitrary functions of $\alpha$ and $\beta$. It may be remarked that $\mathbf{M}.\mathbf{M} = 0$ and that $\theta$ is a solution of the wave equation.

In all the fields of the above type electricity or magnetism travels along straight lines with the velocity of light (the case of a plane wave of light is, however, an exception). To obtain fields in which electricity or magnetism travels with a velocity less than that of light we must superpose fields of the above type in such a way that there is a cancelling of nearly all the concentrated electric or magnetic charges. It is fairly easy to prove that the field of an isolated electric pole moving with a velocity less than that of light can be regarded as the limit of two superposed radiant fields of the type obtained by subtracting two of our elementary solutions. According to this idea the electricity at an electric pole is continually being renewed, moreover, it is the electric charge itself which is directly responsible for the effects produced at a distance, but to understand fully the production of these effects we must consider how this charge is constituted remembering how the field was built up from four of our elementary fields. It is important to notice in this connection that the volume density of electricity and electric current in an elementary field are independent of the direction in which the concentrated charges move; this simplifies matters when we want to ascertain the structure of the aether for an electric pole that is built up in a specified way.

When a number of radiant fields are superposed a cancelling of concentrated electric charges seems to be necessary in order that the principle of the conservation of energy may hold and in order that steady states of motion may be possible. Even if the cancelling is not complete everywhere it must at least be sufficient to prevent any free electricity from going to infinity. If this less stringent condition is adopted it is possible to admit fields in which the charge associated with an electric pole fluctuates slightly and electric charges are fired from one electric pole to another. It may be possible to explain gravitation in this way for it will soon be realised that the necessary fluctuation of charge is exceedingly small. The question may also be raised whether it is necessary for there to be a complete cancelling of the charges in the elementary aethers when a number of elementary fields are superposed. If the answer is in the negative the volume densities of electricity and convection current will be derivable from a function $\Psi$ which satisfies the wave-equation at points not occupied by matter.

Summing up the essential features of the present theory, we may say that all electric charges are supposed to really travel along rectilinear paths with the velocity of light; this implies that when electricity appears to move with a smaller velocity it is made up of different entities at different times being constantly renewed so to speak. The fact that an electric charge which has been moving along a rectilinear path with the velocity of light has no surrounding field[2] is quite consistent with the present view, for all electric charge arises from electric separation[3] and its aether is created in the process, consequently an electric charge which has moved along a rectilinear path for all time with the velocity of light would have no aether to support a field.

When we admit the mathematical possibility that the electric charges in the universe have not existed in the free state for the whole of time we find that it is by no means certain that the aether fills the whole of space and this raises some interesting philosophical questions. Some of the logical difficulties[4] in the ideas of contact action and action at a distance are avoided in the present theory because an electric charge at a point $P$ produces an effect at a distance point $Q$ for the simple reason that either a portion of the charge itself, or a portion of the compensating charge that was created at the same time, actually goes to $Q$ and helps to produce the particle that is acted upon, or rather the entity that represents the particle at the moment under consideration.

Passing on to a brief consideration of some more familiar types of fields we shall superpose the fields of point charges using Lienard's potentials

$$\mathbf{A} = -\frac{e}{4\pi}\frac{\mathbf{v}(\alpha)}{\nu}, \qquad \Phi = -\frac{e}{4\pi}\frac{c}{\nu}.$$

We shall be interested chiefly in the effect of an operation analogous to differentiation. Let us suppose that the co-ordinates of the point charge at time $\alpha$ depend on a parameter $\beta$ as well as $\alpha$ and let $\mathbf{b}$ denote the vector with components $\partial\xi/\partial\beta$, $\partial\eta/\partial\beta$, $\partial\zeta/\partial\beta$, then if $\mathbf{f}$ is a function of $\alpha$ and $\beta$ it is easy to prove that

$$\frac{\partial}{\partial\beta}\left(\frac{f}{\nu}\right) = \frac{1}{\nu}\frac{\partial f}{\partial\beta} - \operatorname{div}\left(\frac{f\mathbf{b}}{\nu}\right).$$

When $f = \partial\omega\ (\alpha, \beta)/\partial\alpha$ this formula may be written in a more convenient form by making use of the relations

$$\frac{\partial}{\partial x}\left(\frac{\xi'}{\nu}\right) + \frac{\partial}{\partial y}\left(\frac{\eta'}{\nu}\right) + \frac{\partial}{\partial z}\left(\frac{\zeta'}{\nu}\right) + \frac{\partial}{\partial t}\left(\frac{1}{\nu}\right) = 0,$$

$$\xi'\frac{\partial}{\partial x} + \eta'\frac{\partial}{\partial y} + \zeta'\frac{\partial}{\partial z} + \frac{\partial}{\partial t} = \frac{\partial}{\partial\alpha},$$

where the operators in the last equation are supposed to act on a function of $\alpha$ and $\beta$.

We thus obtain the equation

$$\frac{\partial}{\partial\beta}\left(\frac{1}{\nu}\frac{\partial\omega}{\partial\alpha}\right) = \frac{\partial}{\partial x}\left[\frac{1}{\nu}\frac{\partial(\omega,\xi)}{\partial(\beta,\alpha)}\right] + \frac{\partial}{\partial y}\left[\frac{1}{\nu}\frac{\partial(\omega,\eta)}{\partial(\beta,\alpha)}\right] + \frac{\partial}{\partial z}\left[\frac{1}{\nu}\frac{\partial(\omega,\zeta)}{\partial(\beta,\alpha)}\right] + \frac{\partial}{\partial t}\left[\frac{1}{\nu}\frac{\partial(\omega,\alpha)}{\partial(\beta,\alpha)}\right].$$

In particular we have

$$\frac{\partial A_x}{\partial\beta} = -\frac{e}{4\pi}\left[\frac{\partial}{\partial y}\left\{\frac{1}{\nu}\left(\eta'\frac{\partial\xi}{\partial\beta} - \xi'\frac{\partial\eta}{\partial\beta}\right)\right\} - \frac{\partial}{\partial z}\left\{\frac{1}{\nu}\left(\xi'\frac{\partial\zeta}{\partial\beta} - \xi'\frac{\partial\xi}{\partial\beta}\right)\right\} + \frac{\partial}{\partial t}\left(\frac{1}{\nu}\frac{\partial\xi}{\partial\beta}\right)\right],$$

$$\frac{\partial\Phi}{\partial\beta} = \frac{ec}{4\pi}\left[\frac{\partial}{\partial x}\left\{\frac{1}{\nu}\frac{\partial\xi}{\partial\beta}\right\} + \frac{\partial}{\partial y}\left\{\frac{1}{\nu}\frac{\partial\eta}{\partial\beta}\right\} + \frac{\partial}{\partial z}\left\{\frac{1}{\nu}\frac{\partial\zeta}{\partial\beta}\right\}\right],$$

Writing $\mathbf{p} = e\mathbf{b}/4\pi$, we find that for an electric doublet of moment $4\pi\mathbf{p}$ the electric potentials are

$$\mathbf{A} = \operatorname{rot}\left\{\frac{1}{\nu}(\mathbf{v}\times\mathbf{p})\right\} - \frac{\partial}{\partial t}\left\{\frac{1}{\nu}\mathbf{p}\right\}, \ \Phi = c\operatorname{div}\left\{\frac{1}{\nu}\mathbf{p}\right\}.$$

This of course is a simple generalisation of the well known result due to Hertz and Righi.

It is well known and easy to verify that the same electromagnetic field may be derived from the magnetic potentials.

$$\mathbf{B} = -c\operatorname{rot}\left\{\frac{1}{\nu}\mathbf{p}\right\} - \frac{1}{c}\frac{\partial}{\partial t}\left\{\frac{1}{\nu}(\mathbf{v}\times\mathbf{p})\right\}, \ \Omega = \operatorname{div}\left\{\frac{1}{\nu}(\mathbf{v}\times\mathbf{p})\right\},$$

with the aid of the formulae

$$\mathbf{E} = \text{rot}\,\mathbf{B}, \qquad \mathbf{H} = \frac{1}{c}\frac{\partial \mathbf{B}}{\partial t} + \nabla\Omega.$$

Let us now write

$$\mathbf{M} = \mathbf{H} + i\mathbf{E} = i\,\text{rot}\,\mathbf{L} \equiv \frac{1}{c}\frac{\partial \mathbf{L}}{\partial t} + \nabla\Lambda,$$

$$\mathbf{L} = \mathbf{B} - i\mathbf{A} = \frac{\partial \mathbf{G}}{\partial t} + ic\,\text{rot}\,\mathbf{G}, \qquad \Lambda = \Omega - i\Phi = -c\,\text{div}\,\mathbf{G};$$

then in the case of an electric doublet we have

$$\mathbf{G} = \frac{i}{\nu}\left[\mathbf{p} + \frac{i}{e}(\mathbf{v} \times \mathbf{p})\right].$$

To obtain the field of a magnetic doublet we write $i\mathbf{q}$ instead of $\mathbf{p}$ for the field of a magnetic pole of strength $\mu$ is derived from the magnetic potentials

$$\mathbf{B} = \frac{\mu}{4\pi}\frac{\mathbf{v}}{\nu}, \qquad \Omega = \frac{\mu}{4\pi}\frac{c}{\nu}.$$

If $\mathbf{m} = \mathbf{q} - i\mathbf{p}$ and

$$\mathbf{G} = -\frac{1}{\nu}\left[\mathbf{m} + \frac{i}{c}(\mathbf{v} \times \mathbf{m})\right] = \frac{\mathbf{g}(\alpha)}{\nu}, \text{ say,}$$

the derived field is that of an electric doublet and magnetic doublet which move together. When the vector $\mathbf{g}$ is given it is easy to determine the moment $4\pi\mathbf{p}$ of the electric doublet and the moment $4\pi\mathbf{q}$ of the magnetic doublet.

The function $\mathbf{g}(\alpha)/\nu$ may be regarded as analogous to the fundamental potential function $1/r$ of electrostatics and Hertzian functions of higher order may be derived from it by differentiation just as potential functions involving spherical harmonics are derived from $1/r$ by differentiation according to a method developed by Maxwell.

Let us regard $\xi$, $\eta$, $\zeta$, $\mathbf{g}$ as functions of $\alpha$ and a parameter $\beta$ then we obtain by differentiation a new Hertzian function whose $x$-component is

$$G'_x = \frac{\partial}{\partial\beta}\left\{\frac{1}{\nu}g_x(\alpha)\right\} = \frac{1}{\nu}\frac{\partial g_x}{\partial\beta} - \text{div}\left\{\frac{\mathbf{b}g_x}{\nu}\right\}.$$

It should be noticed that this expression for $G'_x$ contains differentiation with regard to $x$, $y$ and $z$ but not $t$ so that there is apparently a lack of symmetry. This is due to the fact that $\alpha$ is taken to be independent of $\beta$; we can easily introduce a term involving a differentiation with regard to $t$ by making use of the identity

$$\frac{1}{\nu}f'(\alpha) = \text{div}\left(\frac{f\mathbf{v}}{\nu}\right) + \frac{\partial}{\partial t}\left(\frac{f}{\nu}\right),$$

but it is generally simpler and more convenient to retain the former expression.

The process of differentiation may be carried out any number of times with respect to different parameters using formulae of differentiation analogous to the above. When the various derivatives are added together the result indicates that the natural generalisation of a series of spherical harmonics of form

$$\frac{S_0(\theta,\phi)}{r}+\frac{S_1(\theta,\phi)}{r^2}+\frac{S_2(\theta,\phi)}{r^3}+$$

is the following type of series of Hertzian functions of different orders

$$\mathrm{G}=\frac{\mathbf{g}_0}{\nu}+\operatorname{div}\left(\frac{\mathbf{a}_0\mathbf{a}_1}{\nu}\right)+\operatorname{div}\operatorname{div}\left(\frac{\mathbf{b}_0\mathbf{b}_1\mathbf{b}_2}{\nu}\right)+\operatorname{div}\operatorname{div}\operatorname{div}\left(\frac{\mathbf{c}_0\mathbf{c}_1\mathbf{c}_2\mathbf{c}_3}{\nu}\right)+.$$

Here $\mathbf{g}_0$, $\mathbf{a}_0$, $\mathbf{a}_1$, $\mathbf{b}_0$, $\mathbf{b}_1$, $\mathbf{b}_2$ .... are arbitrary vector functions of $\alpha$. It should be remarked that the vector with suffix $n$ is treated as the vector in forming the $n$ divergence while the other vectors are treated for the moment as scalar quantities. The product of $k$ vectors which occurs in the $(k+1)$th term is to be regarded as a tensor of the $k$th order with $k$ components each of which is a product of components of the separate vectors; there appear to be enough arbitrary functions in a sum of products of this type with $k = 0, 1, 2, \ldots, K$ for the representation of the sum of a number of Hertzian functions up to order $K$.

---

[1] As each shell of electricity moves outwards it induces a secondary separation of electricity, so that electricity flows back to a new position of the primary singularity $(\xi, \eta, \zeta)$ and tends to maintain the electric separation. The volume density of the compensating electricity created at the primary singularity isthus not $\rho$ but is proportional to $\Psi/r$, it is this electricity which is regarded as forming the elementary æther associated with the primary singularity and it is this electricity which, on account of its displacement from the concentrated charge, is directly responsible for the field.

[2] See for instance Wilson, E. B., *Washington Acad. Sci.*, **6**, 1916, (665–669).

[3] Larmor, J., *London, Proc. Mathe. Soc.*, **13**, 1913, p. 51.

[4] Whitehead, A. N., *The Anatomy of some Scientific Ideas, The Organization of Thought*, London, 1917, p. 182.

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# *INVARIANTS WHICH ARE FUNCTIONS OF PARAMETERS OF THE TRANSFORMATION*

By Oliver E. Glenn

Department of Mathematics, University of Pennsylvania

Communicated by H. H. Donaldson. Read before the Academy, November 21, 1917

A systematic theory and interpretation of invariantive functions which contain the parameters of the linear transformations to which a quantic

$f_m$ of order $m$ is subjected has not been formulated, although a paper on invariants[1] published in 1843 by Boole treated certain functions of this type. These were the concomitants of forms under transformations which rotate cartesian axes inclined at an angle $w$ into another set with inclination $w'$; invariants which contain the parameter $w$. Orthogonal concomitants, which are special cases with $w = \frac{1}{2}\pi$, were made the subject of a number of later papers[2], notably by Elliott and by MacMahon.

I have considered a general doctrine of such concomitants for the transformation with four parameters

$$T\colon\ x_1 = \alpha_1 x_1' + \alpha_2 x_2',\ x_2 = \beta_0 x_1' + \beta_1 x_2';\quad D = \alpha_1\beta_1 - \alpha_2\beta_0 \neq 0.$$

The elements of the methods are based upon the two forms

$$\xi = 2\beta_0 x_1 + (\beta_1 - \alpha_1 + \Delta)x_2,\quad \eta = 2\beta_0 x_1 - (\beta_1 - \alpha_1 - \Delta)x_2;$$

whose roots are the poles of $T$, and the expansion of $f_m$ in terms of $\xi$, $\eta$ as arguments. The quantity $\Delta$ employed here is the square root of the discriminant of the form

$$J\colon \beta_0 x_1^2 + (\beta_1 - \alpha_1)x_1x_2 - \alpha_2 x_2^2.$$

The coefficients $\varphi_{m-2i}$ $(i = 0, \ldots, m)$ in the expansion of $f_m$ are invariants of a new type[3] belonging to the domain $R(1, T, \Delta)$ of rational polynomials in the coefficients of $f_m$ and those of $T$, increased by adjunction of $\Delta$. These invariants compose a fundamental system in $R$. They satisfy the invariant relations

$$\varphi'_{m-2i} = \rho^{m-2i} D^i \varphi_{m-2i}\ (i = 0, \ldots, m), \tag{1}$$

in which $\rho$ is one of the two factors of $D$ in $R$:

$$\rho = \tfrac{1}{2}(\alpha_1 + \beta_1 + \Delta). \tag{2}$$

When one seeks complete systems for the given domain $R(1, T, 0)$, free from $\Delta$ but including rationally the coefficients of $T$, it is found that the concomitants are in one to one correspondence with those invariantive products

$$P = \prod_{i=0}^{m} \varphi_{m-2i}^{x_i}\, \xi^{\alpha}\, \eta^{\beta}$$

for which the exponent of $\rho$ in the invariant relation $P' = \rho^a D^b P$, is zero. The conclusion is then drawn that concomitants in $R(1, T, 0)$ are in one to one correspondence with the solutions of the diophantine equation

$$\delta \equiv \sum_{i=0}^{m} x_i(m - 2i) + \beta - \alpha = 0. \tag{3}$$

This infinitude of concomitants therefore forms a system which possesses the property of finiteness,[4] and the fundamental invariants are furnished by the finite set of irreducible solutions of $\delta = 0$.

In the ternary realm the lines joining the three poles of the transformation $T$ on three variables furnish three linear forms in terms of which any quantic $f_m$ in three variables can be expanded. The coefficients in this expansion urnish complete systems in each of several domains. In particular, if $T$ is the transformation which rotates cartesian axes in three dimensional space,

$$\begin{aligned} x &= l_1x' + l_2y' + l_3z', \\ y &= m_1x' + m_2y' + m_3z', \\ z &= n_1x' + n_2y' + n_3z', \end{aligned}$$

the coefficients being the well-known direction cosines of three axes, the invariant triangle on the poles consists of the lines

$$\begin{aligned} f_{\pm 1} &\equiv (l_3 + n_1 e^{\pm i\theta})\, x + (m_3 + n_2 e^{\pm i\theta})\, y + (n_3 - \overline{l_1 + m_2}e^{\pm i\theta} + e^{\pm 2i\theta})\, z = 0, \\ f_0 &\equiv (l_3 + n_1)\, x + (m_3 + n_2)\, y + (n_3 - l_1 - m_2 + 1)\, z = 0, \end{aligned}$$

where $\theta$ is a definite auxiliary angle. The coefficients in the expansion of $f_m$ in the arguments $f_{\pm 1}$, $f_0$ are invariants belonging to the domain of complex numbers, while the finiteness of complete systems in the real domain is determined, and the fundamental concomitants are given, by the finite set of irreducible solutions of the linear diophantine equation

$$\sum_{i=0}^{m} \sum_{j=0}^{m-i} x_j^{(i)} (m - i - 2j) + \beta - \alpha = 0. \tag{4}$$

*The Invariants of Relativity.*—Among numerous important particular cases of the above theory is the transformation of space and time coordinates in the theory of relativity, known as the transformation of Einstein.[5] This consists of

$$T_1: \;\; t = \mu\,(c^2t' + vx')\,/\,c, \;\; x = \mu\,(vt' + x')\,c, \;\; y = y', \;\; z = z',$$

where $\mu = 1/(c^2 - v^2)^{-\frac{1}{2}}$, $t$ is the time, $c$ the velocity of light, $v$ the relative velocity of the moving systems of reference and $x$, $y$, $z$ space coördinates. For these unitary substitutions, $\xi = ct + x$, $\eta = ct - x$ and the invariant relations are

$$\xi' = \rho\xi, \; \eta' = \rho^{-1}\eta,$$

in which (Cf. (2))

$$\rho = \sqrt{c - v} \,/\, \sqrt{c + v}.$$

We now find

$$J: c^2t^2 - x^2,$$

this being an absolute universal covariant of $T_1$ for all values of the relative velocity $V$.

A binary form $f_{1m}(t, x)$ in $t$ and $x$, whose coefficients are constants or arbitrary functions of the quantities left fixed by $T_1$, has a finite system of non-absolute invariants corresponding to the forms in the system for $f_m$ belonging to $R$ (1, $T$, $\Delta$), and a finite system of absolute concomitants analogous to the system for $f_m$ in the domain $R$ (1, $T$, 0). To obtain the concomitants of $f_{1m}(t, x)$ one may either particularize those of $f_m$ under $T$, making the substitutions which reduce $T$ to $T_1$, or, the invariants under Einstein's transformations can be developed *ab initio* by the methods described above for $T$, the arguments of the expansion of $f_{1m}$ being now $\xi = ct + x$, $\eta = ct - x$. These invariantive functions represent invariant loci in four dimensional space if the time $t$ is interpreted as a fourth dimension. All are free from $v$.

A paper in which the above theory and applications are developed in detail and which includes tables of the relativity invariants computed for the general $f_{1m}$ in the case of the non-absolute systems, and for the orders 1 to 3, inclusive, in the case of the absolute systems, is to appear in the *Annals of Mathematics*.

[1] Boole, *Cambridge Mathematical Journal*, **3**, 1843, (1).

[2] Elliott, *London, Proc. Math. Soc.*, **33**, 1901, (226).

[3] O. E. Glenn, *New York, Trans. Amer. Math. Soc.*, **18**, 1917, (443)

[4] Hilbert, *Leipzig, Math. Ann.*, **36**, 1890, (473).

[5] Einstein, *Leipzig, Ann. Physik*, **17**, 1905. Lorentz, Einstein, and Minkowski, *Das Relativitätsprinzip*, 1913, p. 27. R. D. Carmichael, *The Theory of Relativity*, 1913, p. 44.

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# *EFFECTS OF A PROLONGED REDUCTION IN DIET ON 25 MEN*

## *I. INFLUENCE ON BASAL METABOLISM AND NITROGEN EXCRETION*

By Francis G. Benedict and Paul Roth

Nutrition Laboratory, Carnegie Institution of Washington, Boston

Read before the Academy, April 22, 1918

A year ago, realizing that this nation faced a food shortage, several members of the staff of the Nutrition Laboratory decided that positive evidence regarding the effect of a prolonged restriction in diet would give knowledge of possible use in an imminent emergency. Such data seemed especially important as exact experimentation on a large number of men and women, including many with peculiar dietetic habits and a supposedly low metabolism, had failed to indicate that the basal or maintenance metabolism of any particular class of persons or, indeed, of any single individual (making due allowance for differences in weight), is materially lower in energy requirement than the basal metabolism of the average individual. Since the law of the conservation of energy obtains in the human organism, it is clear that with uniform maintenance metabolism, the food requirement must also be fairly uniform. On the other hand, no evidence is available as to the actual effect of a reduction in diet, continued over a considerable period of time. Accordingly, a research was carried out by the Nutrition Laboratory during the past winter in which the effect of a low diet upon a group of normal adults was studied for a period of several months.

If the food intake is reduced below the maintenance level and the basal requirement remains constant, it is plain that there must be drafts upon previously-stored body reserves. In any study of the effect of a reduced diet, since we are dealing primarily with the question of energy rather than with a specific ingredient of the food, there must be the strictest control of the diet, so that the exact intake of energy over relatively long periods may be known. This involves, with human subjects, a degree of personal integrity and veracity that cannot be assumed but must be demonstrated.

For subjects we selected 12 young men from a considerable number of volunteers from the student body of the International Y. M. C. A. College in Springfield, Mass. The average age of the men was twenty-three years. For a period of four months these men were kept upon a much restricted diet, with an energy content of approximately one-half to two-thirds of their caloric requirements prior to the test. During the first few weeks there was a distinct drop in body weight. When the body weight had fallen on the average 12%, the calories in the diet were increased so as to prevent further loss in weight. The measurement of the caloric consumption at this sustained weight level would indicate the true caloric requirement for maintenance. For control 12 men from the same student body, living under exactly the same conditions save for the dietetic restrictions, were likewise studied and their food intake occasionally measured for periods of four or five days.

It was impossible to place all the men in respiration chambers and study their metabolism during the entire twenty-four hours, for it was realized that these men were, first, college students, and second, volunteers for scientific experimentation, and that their college duties, both intellectual and physical, must be carried out; hence they were all cautioned at the beginning of the study not to restrict their activities in any way. Even with the best conditions it could not be assumed that the muscular activity of both groups of men would be exactly alike throughout the period of observation. It thus seemed best to make the measurements of the metabolism upon a uniform basis, excluding uncertain and, more particularly, uneven muscular activity. For this purpose all of the first group of men were studied during periods of complete muscular repose and without food in the stomach, so as to obtain the basal metabolism practically every morning from the 27th of September, 1917, to the 3rd of February, 1918. The individual measurements were controlled by a group study with a large respiration chamber in the Nutrition Laboratory in which the 12 men slept every alternate Saturday night; the metabolism during deep sleep was thus obtained. No individual tests were made with the control squad, but group measurements were obtained on the alternate Saturday nights and used for comparison with the group results obtained for the diet squad.

Prior to the dietetic restriction, the basal metabolism, measured inside the large respiration chamber at night, was the same with the first and second squads, thus giving admirable proof that a group of 12 men was sufficiently large for our purpose. In addition to the observations on weight and basal metabolism, records were obtained of the total nitrogen in the food, feces, and urine during the entire four months, frequent observations were made of the pulse and respiration rate, total ventilation of the lungs, alveolar carbon dioxide, energy requirement for walking a definite distance at a definite speed, the blood (including counts of the red and white corpuscles and differentials, and determination of the haemoglobin), the blood pressure and rectal and skin temperatures. Clinical examinations were also made, as well as measurements of the body surface, strength tests, and extensive psychophysiological examinations of the neuro-muscular processes. The effects of the prolonged reduction in diet on these various functions were striking. Those dealing primarily with basal metabolism may be summed up as follows:

1. A gradual reduction in weight to a point 12% below the initial weight took place during a period of from three to ten weeks, with low calories and a moderate amount of protein in the food intake. The normal demand of the men prior to the dietetic alteration ranged from 3200 to 3600 net calories. One squad of 12 men subsisted for three weeks on 1400 net calories without special disturbance.

2. After the loss in weight of 12% had been reached, the net calories required to maintain this weight averaged about 2300, or approximately one-third less than the original amount required.

3. At the end of the reduction in weight, the actual heat output during the hours of sleep, as computed by indirect calorimetry, was approximately one-fourth less than normal, thus giving a rough confirmation of the lowered number of calories found by actual measurement of the food intake. That there was no seasonal variation in metabolism was shown by the constancy in the metabolic level of the control squad.

4. The heat output by indirect calorimetry per kilogram of body weight and per square meter of body surface was essentially 18% lower than at the beginning of the study.

5. The analyses of food, feces, and urine were sufficiently complete to permit a nitrogen balance to be made and it was found that throughout the period of loss in weight and for some time subsequent thereto, there was a marked loss of nitrogen to the body. In round numbers these men each lost approximately 150 grams of nitrogen. There is an intimate relationship between this 'surplus nitrogen' and the metabolic level. Removing what we may designate as 'surplus nitrogen,' we believe distinctly lowers the stimulus to cellular activity.

6. The nitrogen output per day at the maintenance diet of 2300 net calories was about 9 grams. A control group of 12 men, living substantially the same life and eating in the same dining room, but with unrestricted diet, showed a nitrogen output of 16 to 17 grams per day.

7. The pulse rate was astonishingly lowered. Many of the men showed morning pulse rates as low as 33 and daily counts of 32, 31, and 30 were obtained; at least one subject gave six definite counts on one morning of 29.

8. The blood pressure, both systolic and diastolic, was distinctly lowered.

9. The skin temperature, as measured on the surface of the hands and forehead, was, with some subjects, considerably lower than normal. With most of the men normal temperatures prevailed.

10. The rectal temperature was practically normal.

The general picture that the men presented at the end of the test or at their minimum weight was one of noticeable emaciation, particularly in the face, but all the men continued the usual college activities with no obvious reduction in stamina.

Two of the men had chronic bad noses. One was operated upon during the test and the other should have been. Aside from these two, the prevalence of colds during the period was about the same as with the other college students. During the study three men underwent ether narcosis for operations (on nose, foot, and hemorrhoids) and made rapid recoveries. One man at the lowest period of weight contracted what was diagnosed by three physicians as typhoid fever, although the final course of the disease seemed to leave the diagnosis somewhat in doubt. He ran a very high fever, and was critically ill for some time, but has made a complete convalescence and recovery and has returned to college.

The most noticeable discomfort experienced by the subjects was a feeling of cold, which it is only fair to say might be due in large part to the severity of the past winter. In general, notwithstanding the very great reduction in the metabolism (which we believe was due to the removal from the body of the stimulus to cellular activity of approximately 150 grams of 'surplus nitrogen'), the whole period of lowered food intake had no untoward effect upon the physical or mental activities of these men, and they were able to continue successfully their college duties.

The control squad, having demonstrated the absence of a seasonal variation in metabolism for about three months, were put for a period of three weeks upon a restricted ration of less than one-half their previous calorie consumption. In all details the picture exhibited by the first squad was strikingly duplicated by the second squad, although, as the loss in weight was obviously not so great (6% as compared with 12%) the phenomena were quantitatively somewhat less emphasized.

The entire research will be published in conjunction with our co-workers, Drs. Walter R. Miles, and H. Monmouth Smith, in a monograph of the Carnegie Institution of Washington in the near future.

# EFFECTS OF A PROLONGED REDUCTION IN DIET ON 25 MEN

## II. BEARING ON NEURO-MUSCULAR PROCESSES AND MENTAL CONDITION

By Walter R. Miles

Nutrition Laboratory, Carnegie Institution of Washington, Boston

Communicated by F. G. Benedict. Read before the Academy, April 22, 1918

It is obvious that any adequate investigation of prolonged reduction in diet must include observations of the neuro-muscular processes and general mental condition of the individuals studied. In the low-diet research the psychological measurements were made at the Nutrition Laboratory on Saturday evenings and Sunday mornings when the men were in Boston for the bi-weekly group metabolism experiment in the large respiration chamber.

After a standard evening meal the subjects came to the laboratory and were available during a period of approximately four hours. Of this time about one hour was spent on measurements which could be given to the men as a group. This was done in the library where table space and other conditions were adequate. Particular attention was given to the matter of lighting. The men were assigned seats which they occupied at each session although it was not believed that one location in the room was more favorable than another for the group experiments. A considerable period was allowed for preliminary adjustments, announcements, questions and a general quieting of the men before beginning the evening session.

The first measurement was a test of accuracy or steadiness of movement in tracing between parallel lines with many right-angle turns. The tempo was set by the metronome beating half seconds. The subject was instructed to start at a signal, to avoid contact with the boundary lines, and to make one straight line for each beat of the metronome. Each contact with a boundary line was counted an error.

Other measurements which could be suitably given by the group method were: Discrimination for the pitch of tones, using tuning forks and resonators as source (150 judgments); discrimination and cancellation of specified number groups on a printed page of numbers, the task being to mark each pair of successive digits which, when added, equaled 11; the addition of one-place numbers arranged in columns of ten during a test period of ten minutes; and the memory span for lists of 4-letter English words pronounced in tempo and without stress.

*Psychological measurements of men on reduced diet*

*Group Method:*

1. Accuracy in tracing between irregular parallel lines.
2. Discrimination for the pitch of tones.
3. Discrimination for specified number groups on a printed page.
4. Addition of one-place numbers for a period of ten minutes.
5. Memory span for 4-letter English words.

*Individual Method:*

6. Strength of grip; hands tested alternately.
7. Changes in pulse rate occasioned by short periods of exertion.
8. Latency, amplitude and refractory period of the patellar reflex.
9. Reaction time for turning the eye to a new point of regard.
10. Reaction time for speaking 4-letter words.
11. Continuous discrimination and reaction in finding serial numbers.
12. Sensory threshold for vision.
13. Sensory threshold for electric shock.
14. Speed and accuracy of the eye movements.
15. Number of finger movements performed in ten seconds.
16. Efficiency in traversing a right-angle maze.
17. Speed and accuracy in performing certain clerical tasks.

The measurements taken by the 'individual method' employed much technical apparatus. The general nature of these observations is indicated above. The apparatus was distributed in three rooms, which opened off a common hallway, and with the aid of two assistants it was possible to make psychological measurements with four of our subjects at the same time, each man serving a total of about seventy minutes. The measurements were grouped according to expediency. Nos. 6, 11, 16 and 17 (strength of grip, finding serial number, traversing the maze, and the clerical tasks) were such that two subjects could work in one room without disturbing each other. Nos. 8, 10, and 15 (patellar reflex, word reactions, and finger movements) formed a convenient group for a second room, since they all relied on the Blix-Sandstrom kymograph for chronographic record. The thresholds for vision and electric shock were determined in the third room, the main psychological laboratory. In this room, also, the distinctive measurements for the morning sessions were made. These included Nos. 7, 9, and 14, i.e., changes in pulse rate with exertion, reaction time of the eye, and speed of eye movements, together with more determinations of strength of grip and finger movement speed.

Most of the techniques used in the low-diet research, particularly those given by the individual method, had been previously elaborated in connection with other problems with the object of securing measurements and procedures which could be repeated on the same individual without large practice changes. Great care has been taken to make the measurements as objective and free from personal bias as possible; it will be noted that most of them have a distinct physiological trend. The avoidance of all practice effect and influence from changes in interest and attention is an ideal which of course is never quite reached in psychological investigation; however, the fortnight interval between sessions was a favorable circumstance for minimizing these factors in the present work.

Careful instructions preceded each measurement, both group and individual, and it is a pleasure to record the remarkable coöperation and serious attitude of the men who served as subjects.

It has not been possible thus early to get all the data into final form for comparison. Pending this it is unwise to make dogmatic statements. Final results are available for some of the measurements and the samples given in the table are thought to be representative. Separate averages for the two groups of men are presented in parallel columns headed 'D. S.' (Diet Squad; these men were on low diet from October 4, 1917, until February 3, 1918) and 'C. S.' (Control Squad; these students continued their normal diet up to January 8, 1918, and were then on reduction until January 28, 1918). The values in black face type are under normal condition of diet.

*Illustrative results from psychological measurements during prolonged reduction in diet*

| MEASUREMENTS | NO. 1 TRACING | | NO. 4 ADDITION | | | | NO. 6 GRIP | | NO. 14 EYE MOVEMENTS | | NO. 15 FINGER MOVEMENTS | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Average errors per line | | Total columns in 10 minutes | | Per cent of error | | Average kilos for both hands | | Average time in sigma | | Number in 10 seconds | |
| | D. S. | C. S. | D. S. | C. S. | D. S. | C. S. | D. S. | C. S. | D. S. | C. S. | D. S. | C. S. |
| Experiment lasted 4 months September 29, 1917, to February 3, 1918 | 5.7 | 5.6 | 47.3 | 47.5 | 21.5 | 19.7 | | | | | 68.7 | 66.0 |
| | 5.9 | 5.6 | 50.2 | 46.9 | 16.3 | 10.9 | 47.6 | 52.8 | 95 | 92 | 65.6 | 65.2 |
| | 6.0 | 4.6 | 51.4 | 48.7 | 19.0 | 7.8 | 48.1 | 54.3 | 97 | 96 | 66.6 | 65.6 |
| | 5.4 | 4.7 | 54.0 | 49.6 | 19.9 | 10.8 | 46.9 | 52.8 | 97 | 91 | 64.2 | 64.7 |
| | 5.3 | 4.8 | 52.6 | 42.5 | 15.1 | 9.6 | 45.7 | 53.5 | 98 | 92 | 64.3 | 66.5 |
| | 4.1 | 4.4 | 52.7 | 46.9 | 17.0 | 14.0 | 44.9 | 54.1 | 100 | 95 | 63.4 | 64.2 |
| | 4.7 | 3.5 | 52.8 | 48.7 | 18.4 | 10.7 | 46.2 | 53.2 | | | 64.9 | 62.4 |
| | 4.4 | 3.9 | 52.8 | 51.1 | 14.2 | 8.7 | 45.9 | 53.3 | | | 65.4 | 61.4 |
| | 3.3 | | 53.4 | | 13.2 | | 45.8 | | | | 63.5 | |
| | 3.3 | | 56.8 | | 11.7 | | | | | | 67.6 | |

Accuracy of movement in tracing between parallel lines shows a gradual improvement as the experiment progresses. The diet squad did slightly poorer in the first two sessions after the beginning of reduction and their errors are a little larger than found with the control squad up to December 8. The performance of the control squad, however, continues to improve when on the low diet, hence no definite effect is established.

As would be expected, the efficiency in adding one-place numbers gradually improved. The change, however, is remarkably small. When plotted, the curve for total number of columns added in ten minutes is almost a straight line. The most prominent fluctuation is with the control squad on their last normal date, when it so happened that the best adder (a man who had formerly been a bank clerk) was absent from the group work. The percentage of error is larger for the diet squad and contrasts with the other group in that it fails to decline proportionately as much. On the first reduction day (January 13, the men had been on 1400 net calories per day since January 8) for the control squad there was an increase in error.

In strength of grip the diet squad are definitely below the control, but the three reduction dates for the latter squad show no definite change in strength. Therefore the different level between the two squads cannot be interpreted as due entirely to the low diet condition. Unfortunately it was impossible to begin all the tests on the same date, since preliminary instruction required considerable time at the first session, and no normal is here available for the diet squad.

The eye movements, a type of muscular coördination which is peculiarly independent of voluntary control, show for the diet squad a progressive reduction in speed amounting in all to about 5%. The normal performance of the control squad was not regular, which detracts from the significance of the lengthening in the compared two low-diet sessions.

The diet squad made their best score in finger movements on their one normal date, and from this there was a drop of about 6%, which continued irregularly until January 26. The performance of the control squad is fairly even and at a lower level than the normal for the other men and it shows also a decline on the three low-diet dates.

The many extra-laboratory observations of the men, their subjective impressions, and general physical and mental performance are of interest. Statements from the diet squad men were commonly such as these: "I feel fine, except that I notice the cold, am weak in the legs and seem to lack 'pep;'" "I find it best to study immediately after my meals, and when I begin to feel hunger, I occupy myself with the typewriter or other light physical work;" "The reduction in diet would not be hard to undergo if everyone else at the college were doing it." Opposite the impression of weakness in the legs may be set the record of one of the men, who after being on reduced diet for fifty-six days, led a hare and hound race over a course of approximately 8 miles, running the distance in fifty-four minutes, winning the race by eleven minutes. This man stated that he never felt better after a long distance run.

The men are said to have satisfactorily kept up their college work during the period. And while it might be criticised that the work would be judged leniently by those who were watching the experiment, it should be emphasized that the experiments themselves, the bother of complete collection of urine and feces, the many early morning respiration measurements, and the frequent trips to Boston, made a heavy draft on the time and energy of the men, which their fellow students did not have to bear.

On February 1, 1918, eleven of the diet squad were pitted against 11 selected men from the College at Springfield and placed in an arm-holding contest for endurance. The arms were held extended, palms down, at the level of the shoulders. The number of men falling out were practically the same in both squads; as a matter of fact, 7 of the diet squad and 8 in the competing squad were still holding their arms out in the prescribed manner sixty minutes after the trial started. It was not anticipated that the contest would be so long, and other engagements made it necessary to stop at the end of the hour. Whatever one may think of this as a real test of endurance, it is significant that the men who had been on reduced diet for four months were not appreciably inferior.

Basing judgment on the more objective laboratory measurements, in general it must be concluded that a prolonged reduction in diet produces some decline in neuro-muscular activities, but this does not seem nearly as definite nor as large as the changes in metabolism and allied measurements. The psychological changes were not such as to materially interfere with a satisfactory discharge of the common duties of student life.

# EFFECTS OF A PROLONGED REDUCTION IN DIET ON 25 MEN

## III. INFLUENCE ON EFFICIENCY DURING MUSCULAR WORK

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Communicated by F. G. Benedict. Read before the Academy, April 22, 1918

Of the various forms of muscular activity which might have been chosen for a quantitative study of the efficiency of the human machine under a prolonged period of reduced diet, that of walking was selected since it is the most common and necessary exercise, the element of training is practically negligible, and the results can be expressed in common and well understood terms.

The principle employed in this study consisted of the measurement of the gaseous exchange during the period of walking, and from the oxygen consumed and its known heat value for the determined respiratory quotient the energy requirement was calculated by indirect calorimetry.

The heat requirements thus found may be considered in two ways: (1) as the total cost to the individual to walk a given distance including that fraction of heat which is necessary to maintain the body organs at their normal functions and irrespective of the differences in the amount of body weight transported, or (2) it may be considered on the basis of the cost to the individual to move a unit mass of body weight a unit distance, over and above the cost for the maintenance of the body at some selected basal condition, such as lying or standing quietly. In other words it would be the cost of the superimposed work of walking. In reporting the energy requirement by the latter method it is necessary to know this resting or basal metabolism as well as that of walking.

The problem then was to determine the gaseous exchange of the subjects, at rest and walking, both before and after reduction of diet, and from the differences in the heat developed compute the average energy expended for each man under the different conditions.

Of the various methods of determining the gaseous exchange, the closed chamber principle with gas analyses for carbon dioxide and oxygen at the beginning and end of the experiment was selected for the walking experiments.

Accordingly an air-tight sheet-iron chamber was built large enough to enclose a power-driven treadmill with a man walking on it. The chamber was irregular in shape with a total volume of approximately 2400 liters. Suitable arrangements were installed for controlling the speed of the mill and recording the distance traveled and the number of steps taken.

The temperature of the chamber was determined by six resistance thermometers suitably placed and connected in series with a Wheatstone bridge and galvanometer placed in an adjoining room. Two electric fans stirred the air within the chamber while a blower capable of moving 1000 liters of air a minute carried the air through a pipe extending from the front of the chamber to a drier and thence back into the rear of the chamber. In this pipe of moving air was placed the psychrometer and from it the gas samples were withdrawn for analysis.

During the period of walking pulse records were continuously made by means of electrocardiograms.

The carbon dioxide was determined in duplicate by means of two Haldane portable gas analysis apparatus, while the oxygen was found by means of the Sondén apparatus.

Two groups of 11 and 12 men respectively were under investigation. One squad, designated as the diet squad, was on a reduced ration from October 4 to February 3, while the other squad, known as the control squad, acted as a control until January 8 when it also went on a reduced diet until January 28.

The first experiment was made on January 6 with the 12 men of the control squad two days before their reduction of diet began. Each man walked in the chamber for twenty-four minutes at a rate close to 69.5 meters per minute. After a preliminary period of four minutes, during which time the necessary adjustments were made, the experiment proper began and continued for twenty minutes. The gas samples were drawn at the start and end of this twenty-minute period and from the increase in the percentage of $CO_2$ and the decrease in the percentage of $O_2$ the respiratory quotient was found and the energy expended was computed.

TABLE 1

REDUCED DIET AND HEAT REQUIRED PER MAN DURING HORIZONTAL WALKING, RATE 69. METERS PER MINUTE (2.5 MILES PER HOUR)

| SQUAD | CONDITION | TOTAL HEAT REQUIRED PER MAN PER KILOMETER | HEAT REDUCTION FROM NORMAL |
|---|---|---|---|
| | | *cal.* | *per cent* |
| Control | Normal | 62.2 | |
| Control | 20-day diet | 53.5 | 14 |
| Diet | 4-month diet | 48.4 | 22 |

The average total heat, thus measured, figured on a kilometer basis, is given in table 1 and amounts to 62.2 calories.

After this squad had been on a reduced diet for twenty days the experiment was repeated and it was then found to cost each man on an average 53.5 calories per kilometer. No experiment was made with the diet squad in September as the chamber was not in readiness at the beginning of the study, but on February 3 after four months' dieting an experiment made under the same conditions as the two preceding showed an average expenditure of 48.4 calories per kilometer.

As the average normal weights of the members of the two squads before dieting began differed from each other by only 0.5 kilogram, and since the basal metabolism of the two squads as determined in a large respiration chamber under normal conditions was alike, a comparison of the three sets of experiments may fairly be made. This shows a drop in the energy expended per man per kilometer of 8.7 calories for the twenty-day diet and of 13.8 calories for the four months' diet.

That this drop in the energy requirement is not wholly due to the fact that there was less body weight to be moved may be seen by considering the heat requirements from the second viewpoint, viz., on the basis of a kilogram of body weight transported one horizontal meter.

In considering the energy requirements on this basis, it is usual to deduct from the total heat measured that portion which may be ascribed to the basal requirements of the body in a position of rest. Therefore, in addition to the series of walking experiments just described there was also conducted a series of experiments while the subjects were standing quietly. These were carried out with a portable respiration apparatus in an adjoining room immediately preceding the walking experiment. The difference between the standing and walking metabolisms may then be attributed to the superimposed requirement due to walking, and from the distance walked in unit time and the body weight, the requirement per horizontal kilogrammeter may be obtained.

TABLE 2

REDUCED DIET AND HEAT REQUIRED PER KILOGRAM OF BODY WEIGHT IN WALKING 1 HORIZONTAL METER

| SQUAD | CONDITION | HEAT PER HORIZONTAL KILOGRAMMETER | GAIN OVER NORMAL |
|---|---|---|---|
| | | *g.—cal.* | *per cent* |
| Control | Normal | 0.610 | |
| Control | 20-day diet | 0.561 | 8 |
| Diet | 4-month diet | 0.522 | 14 |

The three sets of experiments figured upon this basis are given in table 2. Here is shown a decreasing energy requirement from 0.610 gram calorie per horizontal kilogrammeter for the control squad in normal condition to 0.522 gram calorie for the diet squad after subsisting four months on a diet of about two-thirds normal. This reduction in the energy from 0.610 to 0.522 gram calorie per horizontal kilogrammeter amounts to an increase in efficiency of approximately 14%.

The results of these experiments are quite positive and show a marked saving in the energy requirements for walking in favor of the reduced diet whether considered on the basis of the gross energy expended which represents the real cost to the individual and to the national food reservoirs, or on the basis of the energy required per horizontal kilogrammeter. Although the results here submitted are confined to one form of muscular activity nevertheless it is believed that the quantitative results obtained would be duplicated if other forms of muscular work were studied.

# POSSIBLE ACTION OF THE SEX-DETERMINING MECHANISM

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Communicated by H. H. Donaldson. Read before the Academy, November 20, 1917

Sex is the dual expression in organisms of a common series of characters which are unequally and reciprocally developed in the individuals of the two classes, the essential and primary difference between which is the presence of germ cells of two types—ova in the female, sperm in the male. Sex is not a necessity for reproduction or for biparental inheritance: it *is* required in reproduction of highly complex organisms. It exhibits many modifications of appearance and intensity in different groups and ranges from apparent correspondence of the two classes to wide extremes of dimorphism. While this gradation prevails for organisms in general, the individual is usually fixed in its status of differentiation. In exceptional cases the individual may possess the primary characters of both sexes or may alternately exhibit one or the other. The measure of difference in any case is cellular. The first observable cellular difference consists in a differentiation of conjugating cells into larger passive food-laden ova, and smaller, active sperms. Similarly the individual, ripe germ cell of either class appears definitely fixed in its character. The primary measure of difference between the two types of germ cells is now demonstrated to be nuclear, and, specifically, chromosomal. Sex is thus shown to be a cellular problem.

Any conception of the sex determining mechanism must conform to the different conditions of range and intensity of sexuality manifested by organisms. As the conditions in one type of sexuality can not be substituted for another, so the character of the determinant in one case can not be implied directly in another. The details of the mechanism are variable in correspondence with the conditions it determines.

In one type, in which the relation was first determined, the alternative character is definitely fixed and the body cells are individually and independently male or female in type. The mechanism of determination here shows itself to be characterized by a difference of one chromosome more in the female than in the male. This is a particular chromosome, marked by such peculiarities in the male as to make it readily determinable. Since this element is the measure of the differences between the male and female types of cell, and therefore the measure of the difference between the male and female animal, a study of its differential behavior should be of value in arriving at conclusions regarding the nature of sex and the method of its determination.

The two principal observations upon which an explanation of the operation of the sex determining mechanism must rest are (1) the duplex and alternative chromosome series, paralleling the double control and alternative inheritance of characters, and (2) the physical state of the active chromosomes. Sex, being a case of strict alternative heredity, would necessarily require a method of control which should unfailingly operate to produce one or the other condition, and, since the numbers of males and females are approximately equal, it should also conform to this numerical requirement. The presence and behavior of the accessory chromosome supplies the theoretical demands of such a control. It is exactly alternative in its apportionment to the male cells, producing two equal and differentiated classes; and, aside from their unequal speed of approach to the egg, and selective attraction by it, should go into exactly half the ova. This behavior has been determined accurately and is no longer questioned.

The alternative mechanism is evident in the behavior of the accessory chromosome. It has not been clear why there should be a double condition of the sex chromosome in the female—in this respect conforming to the ordinary conditions—and a single representation in the male. The condition obtains however and in this fortunate circumstance we find the most promising approach to an understanding of the essential character of sex and the method of its determination. Of great significance here is the fact that, in the mechanism of the germ cells, there exists nothing of functional value in one sex that is not contained in the other. For the differential element of the problem we have therefore to look, not for something that is in itself male or female, but for some factor which, in operation under one set of conditions, will so control a series of characters as to give it the aspect which represents maleness; under another set of conditions the alternative state of femaleness. How then does this differentiator function?

The simplest conception of its action, perhaps, would be to ascribe to it some specific power which, exerted to a certain degree, might eventuate in the aspect of maleness, while in double that effect the series of characters would be female. In essence this is the quantitative theory. It does not however conform to any of our ideas of the alternative, or allelomorphic, action of the chromosomes. What our experience with regard to chromosome action dictates so far is this: One control for a character is sufficient for its elaboration; two controls do not exhibit the sum of their action but find themselves in opposition and usually one or the other prevails (probably for the single cell one always does); additional controls are apparently without effect.

Genetic experiments also show that in cases of sex-linked inheritance a single control in the male has the same effect as a double identical control in the female, and that, if the factors in the female are opposed to each other, such a single control as is fully effective in the male is then only half as potent.

The evidence accordingly relates to conditions of cell constitution and explanations must be in terms of cell organization and known functions. In this respect a comparison of the sex chromosomes of the male and female at the critical time when their germ cells are being prepared for union is most instructive. While in the female there is apparent no unusual activities of the sex element, as compared with the other chromosomes, in the male there are well marked differences both in time and degree of its action. If the results of the activities of the cell are due to the ordered interaction of its parts, then any change in the rate of any one of its controlling elements, or any modification of its time of action, must essentially affect the product. The sex-chromosome during the development of the male germ cells exhibits all the signs of such a differential action. In the first stages it is much more active than the other chromosomes, as is evidenced by the large surface it exposes at the time of greatest cellular interaction. Not only is this true but it is more independent, having its substance isolated in a separate region. From this behavior it is clear why it should, in the single condition here, be quite as effective in its action as the duplex element of the female, if similar activities prevail in the body cells.

At a later stage, when it is almost certain that the new relations between controls of characters are being established, the sex chromosome is withdrawn and has its surface reduced to the least possible extent. It gives every evidence of inactivity, and, indeed, at this time has no occasion for action, since it has no homologue with which to react. In cases, unlike the Orthoptera, where there is a member to pair with it, the behavior is much the same and the genetic evidence would indicate the absence of any reaction.

Undoubtedly this is indicative of the real difference between the female and male organization, and, when fully understood, will point to the meaning of sex. It is possible that we have here the explanation of the greater variability of the male, for if a part of the control system is thus withdrawn from action at the time of reorganization it would, almost inevitably, affect the entire result. Here it may be recalled that there has been much dispute regarding the general significance of sexual reproduction, some holding that it is to ensure variation, others that it is to control or prevent it. Since both of these ends must, in some measure, be attained is it not entirely possible that the sexes represent, in part, such a division of labor? In this connection it may be pointed out that the history of the sex chromosome is such as to fit it exactly for the rôle of furthering these two purposes. It passes alternately from the male to the female line, in the one being subject to the relative instability of its unpaired condition, in the other being an orderly member of a balanced series, forced to react with its mate as do the other chromosomes. There is a possibility that in the male, the sex chromosome being unmated, or opposed by an inactive element, may be more free to react with the other chromosomes and in this way change their constitution, being in turn affected by the reaction. By the nature of its transmission it must, after this experience, pass into the female line where its relation to the complex is necessarily different. The contrast in these two conditions is obvious and the interpretation strongly suggested.

Where there is a mate to the sex chromosome in the male the genetic results would indicate that it has no function, and the very fact that it may indifferently be present or absent suggests its inactivity. This element, of all the chromosomes, is confined to the male line and it is possible that its loss of function is due to a lack of variable reaction. It experiences in effect, a most intensive form of inbreeding and shows the characteristic results of such unvarying reactions. For it, there is no opportunity to eliminate greater or less variables, as is the case with the x-element during maturation in the female. The ultimate problem is, of course, to determine why such a difference between homologous elements should exist.

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# *THE STUDY OF THE SEDIMENTS AS AN AID TO THE EARTH HISTORIAN*

By Eliot Blackwelder

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Communicated by J. M. Clarke, March 29, 1918

*Objectives of the Earth Historian.*—We are, of course, still immensely far from our ultimate goal, which is a complete understanding of all the past states and events of the earth, or as Professor Salisbury used to put it, "the complete geographies of all past epochs." Progress toward this unreachable goal will be most favored if the advance is made rather uniformly, all along the front. It is true that such progress is often made by pushing out salients, but the further extension of such salients is usually impossible without corresponding support from the flanks.

In the past we have gone ahead much farther along certain lines in geologic history than along others. The history of life and of faunal succession has been cultivated assiduously for generations and is, on the whole, much better understood than other phases of the subject. Although not so well known in detail, the history of diastrophism is now fairly well blocked out and the mere continuation of studies already under way is likely to afford us in the near future a serviceable understanding of the sequence of major earth movements.

The most backward points in the general advance just now are in two sectors: That of the history of climate, and that of the principles of chronology and correlation.

The importance of climate arises from the fact that it is one of the most powerful factors, if not indeed the dominant factor, controlling not only the sculpture of the land but the nature of the deposits that are made both on land and in the sea.

Secondly, the principles of correlation must be understood better than they are now, before we can bring into their proper time relations the various events and conditions of which the sediments give us record. In spite of the impressions in elementary text books of geology, I think it will be generally admitted, by those who have carefully considered the question, that we do not yet know these principles with satisfactory accuracy. If we did, we should not have such anomalies as the actual interbedding of strata in Montana containing marine invertebrates assigned to the Cretaceous with those containing land plants identified as Eocene; or as the Triassic in Idaho resting conformably on rocks containing only Pennsylvanian fossils. Many other cases of similar perplexity will occur to those who have had much to do with age questions in stratigraphy. All things considered, it seems to me that the improvement of correlation methods and a more general acceptance of these improvements is at the present time the thing most to be desired by the earth historian, for nowadays it is one of the chief causes of friction among us.

What service can the study of the sediments and sedimentary rocks render in connection with these two problems? First it offers the best and most comprehensive means of working out the history of climate. We already understand rather fully the climatic significance of such deposits as beds of coral limestone, of tillite, and of saline formations. We have probably reached a similar comprehension of the red beds, loess, and certain other types, although we are not yet fully agreed among ourselves regarding them. Before long we may expect to know as fully the climatic significance of the coal-bearing gray sediments, and eventually even of most of the marine deposits. For even among the, latter no matter how great the importance of the work of bacteria, algae, foraminifera, and other organisms is, it becomes increasingly evident that the very activities and processes of these organisms are largely dominated by climate, either directly or indirectly, and that they are forced to make a record of climatic changes in the marine sediments to which they contribute. For example, the prevalence of siliceous in place of calcareous ooze in the Antarctic Ocean is probably due to biochemical factors that depend on climate.

The study of climatic history is not only necessary for its own sake as a division of the larger earth history, but it has an important bearing upon the attainment of the other desideratum, namely, more reliable correlations. Climatic changes are widespread in their influence. Some, like the cooling off during the last glacial period, seem to have affected the whole earth. They influence both land and sea deposits, and hence leave their impress on all sedimentary formations. In comparison with the slow progress of geologic events, the effects of climatic change are felt quickly. We seem justified in believing that altered climates do not ordinarily migrate slowly from region to region, and on this assumption it may generally be presumed that the results are essentially simultaneous over large areas. For these reasons climatic changes should serve as very delicate indicators of time relations. They are likely to be especially valuable because their record is most clear in the terrestrial sediments, where fossils—our customary reliance—are apt to be rare or absent.

From the earliest days of geology attempts have been made to correlate strata in different places by means of lithologic character. Many of these attempts have met with either failure or only partial success. In comparatively recent years, however, more refined methods have yielded much better results. For example, Rogers and Stone in their recent study of the Lebo shale of Montana have shown that it can be identified over large areas by its andesitic particles, which record eruptions of volcanos farther west at a single epoch in Cretaceous times.

In the future it will undoubtedly be possible to make far greater use of the physical characteristics of sedimentary rocks in correlation—not so much by a direct matching of similar rocks as by an indirect process of first elaborating from the rocks the climatic and physiographic conditions and their changes in time, and then correlating these. It should in fact be as feasible to correlate by means of climatic history as to correlate by diastrophic history. Indeed it is already beginning to be the practice of the most progressive stratigraphers to make their correlations not simply on the basis of faunas nor on the basis of diastrophism, but on the compound basis of life, climate, topography, vulcanism, and diastrophism with due regard to their mutual relations and dependences and their relative values. It is my own expectation that this practice will soon become general.

*What is Needed.*—We may reasonably hope to understand eventually all of the sedimentary rocks at least as well as we now know any; but at present our knowledge is very uneven, being tolerably complete for some types and very slender for others. Among the sediments which are as yet but partly understood the following may be mentioned by way of illustration of our needs: limestone conglomerates and oolites, sedimentary iron ores, chert, jasper, etc., gray flags and shales with lean faunas, dolomites, phosphorites, greensands, black oily shales, lithographic limestones, and rhythmically alternating shale and limestone. Of course, there are many others.

To advance more rapidly this part of the scientific battle front, several things are needed. Among the most useful are the prolonged and intensive studies of certain modern types of deposits and the processes of their deposition, as illustrated by the investigations that have been carried on recently by T. W. Vaughan and his associates in the Florida region, as well as by the careful study of mountain stream work by G. K. Gilbert in California. Some of these problems are too large to be attacked by most individuals, but require for their successful execution the aid of our strongest scientific institutions and the cooperation of a number of investigators over a period of years.

Hardly less valuable are the close and detailed studies of ancient sedimentary rocks, such as Barrell's interpretation of the Mauch Chunk shale, the study of the western Red Beds by C. W. Tomlinson, and of the dolomites by half a dozen or more geologists in the last few years. Probably this method must be our sole reliance in the case of certain peculiar sediments which, so far as known, are not being formed on the earth at the present time. These are illustrated by the thick, rich beds of phosphorite in Idaho and perhaps by the stratified iron ores of eastern Brazil.

Our understanding can be advanced in a most helpful way also by careful experimentation, such as that on sun-cracks reported by E. M. Kindle, or the bacteriological experiments of Drew and others on the precipitation of calcium carbonate from sea-water and the formation of oolites. On the whole there has been much too little of this sort of work in the last few decades, perhaps because sedimentary studies have been left largely to the stratigrapher who is rarely, by force of habit, a laboratory experimenter. We shall need in this work the coöperative aid of expert chemists, bacteriologists, and others not ordinarily interested in geologic matters. The whole subject of diagenesis, or simultaneous alteration of sediments, is doubtless in the province of the bio-chemists and colloid chemists, if only we could induce them to assume the task.

It might seem, at first thought, superfluous for me to recommend the thorough combing of the geological literature for material on the nature and origin of the sediments. But it is a fact, by no means sufficiently appreciated by most of us, that the amount of buried treasure of this kind is really enormous. A few years ago, while engaged in the study of phosphorite deposits, I was astonished to find that the papers published in France and England during the seventies contained a much clearer and more comprehensive interpretation of phosphatic deposits than could be found in any American text-book or reference work published since 1900. In fact, it was perfectly evident that most even of the more valuable of these foreign papers had never been seen by the authors of the compendia mentioned, nor even by Americans who had published important papers on the phosphatic rocks. I believe it is a fact that one of the greatest services that can be rendered, just now, to the advancement of our knowledge of sediments and sedimentary rocks would be the thorough investigation, digestion, and summarizing of what is already in print on the various types of sediments. It would vastly increase our effective working knowledge and might conceivably double it, for it must be clear that a fact or a theory which is lost is as useless as one that has never been found out.

Chemical analyses give much valuable information regarding sedimentary deposits. Not infrequently the general conditions of origin as well as the nature of the rock, can be inferred at once from the analysis. We have far too few of them, and it is one of the minor discouraging conditions of the study that even those we have are in many instances rendered useless by the omission of essential facts.

Furthermore, a great many analyses are published with only the most meager information regarding the source of the material. It is obviously of very little value to the sedimentationist to know that a certain analysis pertains to a Cretaceous shale from Mt. Diablo, California, because the mountain contains several distinct types of shales of Cretaceous age. In that case even the brief statement that it was a gray sandy shale with abundant fragmentary mollusks and echinoderms would greatly enhance the value of the analysis. The same benefit might be conferred by noting that the specimen came from a particular bed in a carefully described section already published. It is for this reason that the great number of analyses in Clarke's *Data of Geochemistry*, have only a small fraction of the value to the sedimentationist that they might just as well have had if the facts had been adequately stated. We need, therefore, more analyses, more comprehensive analyses, and especially more fully annotated analyses.

Probably our broadest, even if not our deepest, fund of information for the interpretation or sedimentary formations comes from the descriptions of sections by stratigraphers and by field geologists in general. Here, as in the case of the chemical analyses, the sedimentationist meets with frequent disappointment, simply because the exact and detailed observations, which alone could make the section valuable for his purposes, have been largely or entirely omitted. We have thousands of stratigraphic sections in which successive beds are described as 'yellow sandstone,' 'gray shale,' 'fossiliferous limestone,' etc., leaving the reader to guess as best he may the significant characteristics of the strata. Of course, the author of a report usually gives descriptions that are adequate for his own purposes even if not for those of others who may go to it for light. On the other hand those geologists who make it a practice to describe colors carefully, to note textures, the presence of cross-bedding, ripple-marks, sun-cracks, nodules, forms of grains, character of cements, nature, abundance, and distribution of fossils, to say nothing of mineral content, forms of large versus small grains, type and amplitude of cross-bedding and ripple-marks and the nature of the filling in sun-cracks, are a source of joy to the student of the sediments. Unwittingly perhaps, they are rendering us a valuable service. We only regret their scarcity.

Many of us are necessarily limited in the geographic range of our first-hand study of sediments and sedimentary processes. Others who have had the good fortune to visit distant regions, and especially the less known parts of the earth, such as the tropics and the arctic countries, can often with very little trouble collect observations and material which may later, in the hands of a trained student of sedimentation, yield important information for which we might otherwise have to wait for decades. The amount of such data now on hand in American universities and museums is very small when compared with that which is available for a study of volcanic rocks or fossil faunas.

These are some of the ways in which the study of the sediments and the sedimentary rocks can be forwarded not only by the special devotees of that branch of geology but also by anyone who takes an intelligent interest in the matter.

# *THE GROWTH OF THE ALASKAN FUR SEAL HERD BETWEEN 1912 AND 1917*

By G. H. Parker

United States Seal Investigation, 1914

Read before the Academy, April 23, 1918

The apprehension with which the condition of the Alaskan fur seal herd was viewed some half decade ago has disappeared in consequence of the steady growth of the herd. Few animals have been more closely watched and counted from year to year than the Alaskan fur seals and their remarkable habits of breeding exclusively on the Pribilof Islands and of assembling there each summer in one immense complex family render these counts of no small biological interest. It is probable that in late July and early August of each year every living Alaskan fur seal (Callorhinus alascanus) is either on one of the Pribilof Islands or in the immediately adjacent sea. Thus an annual complete rendezvous of this species takes place in an almost unique way, and this rendezvous gives opportunity for a census of the fur seal such as is possible in scarcely any other undomesticated animal.

Table I exhibits in several particulars the numerical conditions of the herd from the summer of 1912, when detailed counting was begun, to that of 1917. The numbers in this table are taken from the successive reports on the state of the herd as given out in the publications of the United States Bureau of Fisheries (Osgood, Preble, and Parker, 1915; Bowers and Allen, 1917; Smith, 1917; *Fisheries Service Bulletin,* No. 30). The counts in the years 1912 and 1913 were made under G. A. Clark; those in 1914 under a group of six investigators, B. W. Harmon, T. Kitahara, J. M. Macoun, W. H. Osgood, G. H. Parker, and E. A. Preble; and those of 1915, 1916, and 1917 under G. D. Hanna.

Table 1 opens with an enumeration of the new-born pups for the seasons under consideration. From 1912 to 1916 inclusive these enumerations were made as direct counts of the numbers of pups on the beaches by methods well established on the islands. In 1916, owing to the increase in the number of pups, direct counting was accomplished only with difficulty, and in 1917, in consequence of still greater increases, it was found necessary to resort in part to a method of estimates. Hence it is believed that the numbers for 1916 and particularly for 1917 are not so accurate as those for the preceding years.

In no feature is the growth of the herd indicated more clearly than in the yearly increase in pups. This increase ranges in a progressive series from 81,984 in 1912 to 128,024 in 1917. The nature of this increase can be appreciated best when the numbers are plotted in some such way as in Graph 1 in which the abscissas represent years and the ordinates numbers of pups in

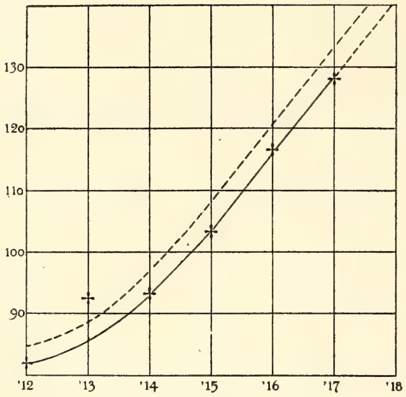
TABLE 1

NUMERICAL STATEMENT OF THE CONDITION OF THE ALASKAN FUR SEAL HERD FROM 1912 TO 1917 INCLUSIVE

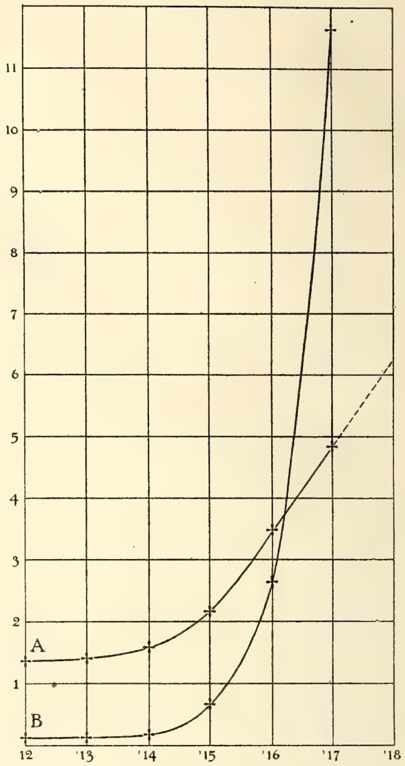
| YEAR | PUPS | HAREM BULLS | AVERAGE HAREM | IDLE BULLS | ESTIMATED TOTAL HERD |
|---|---|---|---|---|---|
| 1912 | 81,984 | 1,358 | 60.4 | 113 | 215,738 |
| 1913 | 92,269 | 1,403 | 65.8 | 105 | 268,305 |
| 1914 | 93,250 | 1,559 | 59.8 | 172 | 294,687 |
| 1915 | 103,527 | 2,151 | 48.1 | 673 | 363,872 |
| 1916 | 116,977 | 3,500 | 33.4 | 2,632 | 417,281 |
| 1917 | 128,024 | 4,850 | 26.4 | 11,683 | 468,692 |

thousands. Here it will be seen that the successive counts, with the exception of one, lie almost exactly on a curve such as would describe the commencement of an autocatalytic process. This type of curve when complete shows in the beginning an accelerated increase, after which it approximates more nearly to a uniform rate till a decline of this rate sets in due to a checking of autocatalysis by retarding factors. The decline ceases when autocatalysis is balanced by the conditions unfavorable to it. This form of curve is usually applied to the growth of an individual, but there is no reason why it should not apply also to the growth of a population, which after all is protoplasmic growth and hence dependent upon autocatalysis. It is, therefore, not surprising to find that the increase in pups should follow a curve characteristic of such a process. The period of accelerated increase, as Graph 1 shows, extended from 1912 to 1914 or 1915. The period over which a more nearly uniform rate of increase was maintained began in 1914 or 1915 and extended to the last reported count, 1917. How much longer it will continue cannot be stated. Eventually, as numbers augment, the beaches on which the breeding occurs will become overcrowded, shortage of food may supervene, epidemics due to unfavorable conditions may appear, and these and other like influences will cut down the rate of increase until the herd, having arrived at its maximum number, will stand at a constant level. Just as it is impossible to predict how long after 1917 the steady increase will be maintained, so also it is impossible to foretell when equilibrium will be reached.

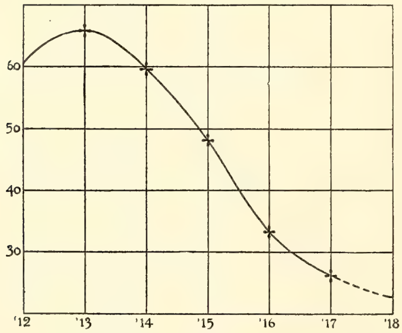
The only pup count that fails to fall in line with the interpretation of the growth of the herd just given is that of 1913. This count when considered in relation to the other counts is some six thousands too high. When the pup census of 1914 was made public, it was pointed out (Osgood, Preble, and Parker, 1915, p. 41) that the increase of this year over the preceding one was very slight, as a matter of fact only a little over 1%, whereas the increase of 1913 over 1912 had been 12.5%. Clark (1916, p. 608) also commented critically on these numbers and stated that "The results gave a gain of only 1%, without any adequate explanation for the irregularity," the implication being that the count of 1914 was deficient. Now that there are in all six counts that may be compared, it is quite obvious, as an inspection of Graph



GRAPH 1



GRAPH 2



GRAPH 3

GRAPH 1. ALASKAN FUR SEAL PUPS BORN IN THE SEASONS 1912 TO 1917 INCLUSIVE

The abscissas represent years and the ordinates numbers of pups in thousands (table 1). The actual numbers counted, except those for the year 1913, fall on a curve represented by the full line which has been extended one year by extrapolation. All counts on the curve are probably low, the dotted curve indicating the direction of their real values.

GRAPH 2. NUMBERS OF HAREM BULLS (A) AND OF IDLE BULLS (B) IN THE ALASKAN FUR SEAL HERD OF THE SEASONS 1912 TO 1917 INCLUSIVE

The abscissas represent years, the ordinates numbers of bulls in thousands (table 1). The curve for harem bulls (A) is extended one year by extrapolation.

GRAPH 3. THE AVERAGE HAREM IN THE ALASKAN FUR SEAL HERD OF THE SEASONS 1912 TO 1917 INCLUSIVE

The abscissas represent years, the ordinates numbers of females in the average harem. The curve has been extended one year by extrapolation.

1 shows, that it is not the count of 1914 that is anomalous but that of 1913. Why this should be so extraordinarily high is difficult to state. It is perfectly clear to any one who has counted pups on the rookeries of the Pribilofs that even the most accurate count is bound to fall short of the real number, so that the magnitudes indicated by the dotted line between 1912 and 1917 in Graph 1 show the directions in which the real numbers lie rather than the solid line of actual enumerations. But even admitting that all the enumerations, except that of 1913, are low, there is no reason to suppose that these are so far low as would be implied on the assumption that the count of 1913 is the most nearly accurate count of them all. It seems impossible that Clark should have improved his method so much between 1912 and 1913 as to have found as large an increase as is implied in the count of 1913. Such increases based on assumed improvement in method do not appear among the several counts by Hanna. But in whatever way this discrepancy in the counts may be explained, it must at least now be clear that the count of 1914 agrees well with the majority of the other enumerations and that the count that is exceptional is that of 1913.

Another way of indicating the growth of the herd as seen in the numbers of pups, is the percentage of annual increase in this constituent. From time to time statements have been made as to what this percentage under normal conditions should be. Thus Clark (1917, p. 499), selecting the increase in 1913 (12.5%) and in 1916 (13.0%), concluded that these figures "may be taken as fixing with reasonable exactness the rate of growth at about 13%." Hanna (Bower and Allen, 1917, p. 92) has expressed the opinion that about 12% is the normal rate of increase. How these numbers compare with the actual figures of the last five years can be seen in table 2.

TABLE 2

INCREASE OF PUPS IN THE ALASKAN FUR SEAL HERD FROM 1913 TO 1917 INCLUSIVE

| YEAR | NUMBER OF PUPS IN THE | | ACTUAL INCREASE | PERCENTAGE INCREASE |
|---|---|---|---|---|
| | GIVEN YEAR | PRECEDING YEAR | | |
| 1913 | 92,269 | − 81,984 | =10,285 | 12.5+ |
| 1914 | 93,250 | − 92,269 | = 981 | 1.1− |
| 1915 | 103,527 | − 93,250 | =10,277 | 11.0+ |
| 1916 | 116,977 | −103,527 | =13,450 | 13.0− |
| 1917 | 128,024 | −116,977 | =11,047 | 9.4+ |
| General average.................................... | | | | 9.4 |

Here it will be noted that the percentage increase varies from 1.1 to 13.0 with an average of 9.4. This computation included the very anomalous count of 1913, but even if this were replaced by a number such as would be indicated by Graph 1 (86,000) such a replacement would not materially alter the general average. The percentage increase in 1913 would then be 4.9

instead of 12.5 and in 1914, 7.8 instead of 1.1 and the general average would be 9.2 instead of 9.4. Thus for the last five years the annual increase of pups has averaged a little over 9% and is probably nearer 9.5 than 9. Hence both Clark's figures and Hanna's are obviously too high for the present state of the herd. In fact Clark's assumption of 13% is quite unwarranted for he gives no reason for accepting this number rather than the other chosen by him, 12.5.

The average rate of increase indicated in table 2, 9.4%, would bring about a doubling of the number of pups in approximately eight years. If no untoward conditions arise, it is fair to expect that in 1920 about twice as many pups will be born as were born in 1912.

Beside the annual increase in the number of pups born, the Alaskan fur seal herd has given evidence of growth in the increase of its harem bulls. These bulls, which are the sexually mature males that have succeeded in having associated with them one or more breeding females, are perhaps the most accurately counted element in the whole herd. The determination of their numbers between 1912 and 1917 was made by direct count. The results of this census are given in the column in table 1 marked 'harem bulls' where it will be seen that they represent an increasing series running from 1358 in 1912 to 4850 in 1917. The distribution of their rates of increase describes a curve (Graph 2, A) not unlike that seen for the pups. An extrapolation on the basis of this curve leads to the prediction that the number of harem bulls in 1918 will be somewhat over 6000.

The average harem for a given season is the average number of females associated with the harem bull of that season. It is a derived number found by dividing the number of females as indicated by their pups for a given season by the number of harem bulls for that season (table 1). If the males of the herd have been considerably reduced in numbers, as might result from excessive killing, the number in proportion to that of the females would be naturally small and consequently the average harem would be large.

Such a condition, if excessive, would be an unfavorable sign in the herd and improvement would be marked by a decrease in the size of the average harem. The course of events in this particular between 1912 and 1917 is represented in table 1 the details of which can be better appreciated by reference to Graph 3. In this it will be seen that the average harem presented its most unfavorable condition in 1913 after which there was a steady improvement to 1917 in that the average number of females to each harem bull fell from 65.8 to 26.4. It is to be remembered, however, that the extremely unfavorable point in the curve, 1913, is dependent upon the anomalous pup count of that year. Had this count been in line with the others, the average harem for 1913 would have been very near those of 1912 and 1914. The subsequent change, however, would not have been affected and this reduction can not be looked upon as anything but a favorable sign. From the extrapolation in Graph 3, there is good reason to believe that the average harem will be even smaller in 1918 than it was in 1917.

The idle bulls, to turn to another element in the herd, are those males that have attained breeding age but that have failed to obtain one or more females. They usually occupy less favorable areas on the outskirts of the rookeries and may even move from place to place. They can be counted with almost as much accuracy as the harem bulls, but their occasional migrations make their count somewhat uncertain. In 1917 the idle bulls were so numerous that it was deemed wise to subdivide them into two classes, idle bulls proper or those with fixed positions on the breeding grounds but without females, and surplus bulls or those that were unable to find positions on the breeding ground and that usually resorted to other parts of the beaches notably the bachelors' hauling grounds. These two classes are combined in table 1, 1917, under the single head 'idle bulls.'

The idle bull indicates a maladjustment in the breeding conditions of the fur seal. This feature has already been pointed out as evidence of imperfect adaptation in this species (Parker, 1915). As already stated, in the fur seal, as in most other higher animals, the numbers of males and females at birth are very nearly equal. When the breeding period arrives, however, one male associates himself with a large number of females, the lowest average harem in the last five years being over 26 females to one male. Consequently, notwithstanding the fact that the male breeding period is only six to eight years (Clark, 1916, p. 608) as contrasted with the longer female period of approximately ten or eleven years, many males do not procure females. Thus the idle bulls are a measure of this natural maladjustment within the herd. In a wholly natural state of the herd they would undoubtedly be represented by considerable numbers. Their presence, at least in large numbers, can never be anything but a detriment. They are continually stirring up strife not only among themselves but also among the breeding bulls and they are accountable for the maiming and the death of many young seals. They are the individuals that in the period of their best pilage should have been killed for their skins and their excessive numbers indicate poor management of the herd. Their history during the period under consideration is shown in table 1 and more strikingly in Graph 2, B. Here it will be seen that the numbers of idle bulls remained small from 1912 to 1915 after which it increased considerably in 1916 and enormously in 1917.

This constitutes the one unfavorable feature in the recuperation of the herd, for it marks the effective appearance on the beaches of the first real element that is detrimental. Fortunately it is within reasonably easy control, for the fur seal herd is open to the same kind of management that chickens or cattle are. In these stocks, as in the fur seals, the sexes are approximately equal. at birth and in both instances, although good management calls for a careful rearing and preservation of females, it also demands the retention of only such males as are necessary for breeding, the excess being drawn off for market purposes. This is clearly what should be done with the surplus male seals, a step that out government is now prepared to take (Smith, 1917, p. 92).

It is only to be regretted that this step was not taken earlier, for the large number of idle bulls now in the herd is both a detriment to that body and a positive loss of once good skins. This menace in the growth of the herd has been repeatedly pointed out (Clark, 1914, 1916, 1917; Osgood, Preble, and Parker, 1915) to those having the matter in charge.

The last column in table 1 contains the estimated annual totals for the herd. These numbers are highly artificial in that they are largely made up of computed elements and they are, therefore, so remote from direct observation that their detailed consideration is scarcely worth while. The fact that all elements of the herd have separately increased between 1912 and 1917 is reflected in the increase of these calculated totals from 215,738 in 1912 to 468,692 in 1917.

In conclusion it may be stated that since 1912 the steady increase in the numbers of pups born, and of harem bulls and the decrease since 1913 of the average harem are most favorable signs in the growth of the herd. The one unfavorable feature during this period is the considerable increase in idle bulls in 1915, 1916, and especially in 1917. This increase, which can be eventually checked, shows that active commercial killing should have been restored some years ago.

Anonymous, *Washington, Fisheries Serv. Bull.*, No. 30, 1917, (6–7).

Bower, W. T., and Aller, H. D., *Alaska Fisheries and Fur Industries in 1916*, *Washington*, 1917, 118 pp.

Clark, G. A., *New York*, *Science*, N. S., **39**, 1914, (871–872); *Ibid.*, **44**, 1916, (608–609); *New York*, *Amer. Mus. J.*, **17**, 1917, (497–499).

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Parker, G. H., *Philadelphia*, *Proc. Amer. Phil. Soc.*, **54**, 1915, (1–6).

Smith, H. M., *Washington*, *Ann. Rep. Comm. Fisheries*, 1917. 104 pp.

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# *THE DESTRUCTION OF TETANUS ANTITOXIN BY CHEMICAL AGENTS*

By W. N. Berg and R. A. Kelser

Pathological Division, Bureau of Animal Industry, Washington

Communicated by R. Pearl, April 13, 1918

The ultimate object of this work is a solution of the problem of the chemical nature of antitoxins and their preparation in the pure state. That this would be attained was not expected in view of the numerous previous investigations which left these problems unsolved. But it seemed highly probable that data would be obtained which would throw some light on the subject.

Up to the present time numerous investigators attempted to separate antitoxins from their associated proteins, but without complete success. The well known tetanus and diphtheria antitoxins are examples of preparations containing all or nearly all of the immunity units present in the original serums, but only a part of the proteins. Thus, Homer[1] (p. 400) concentrated a tetanus serum containing 100 units per cubic centimeter and 6% of protein, obtaining an antitoxin that contained 900 units per cubic centimeter and 19% of protein. In this process 10% of the antitoxic units were lost, the antitoxin was nine times as potent as the serum from which it was derived, but it contained only three times as much protein. The failure of all attempts to obtain a protein-free antitoxin preparation has led some investigators to the conclusion that the antibody or group of antibodies which constitutes the antitoxin is one of the serum proteins and hence cannot be completely separated from protein. The concentration of the antitoxin without a similar concentration of protein is regarded by others as an indication that the antitoxin may be a body of non-protein nature.

Under these conditions any test which would conclusively decide whether an antitoxin is or is not identical with a serum protein, would have both a practical and a theoretical interest. The following test was decided upon because of its promising nature. If an antitoxin, tetanus antitoxin, for example, is a substance of non-protein nature, it should be possible to prepare artificial digestion mixtures containing the antitoxic serum or antitoxin derived therefrom, in such fashion that the protein would undergo digestion without loss of antitoxin. Appropriate chemical measurements would indicate the extent of proteolysis, while inoculation experiments on guinea pigs would indicate whether there was any loss of antitoxic units. If, on the other hand, the antitoxin is a protein and its power to immunologically neutralize the corresponding toxin is a function of the intact protein molecule, then the antitoxin would be destroyed in every case where the protein had undergone cleavage, regardless of whether the cleavage was caused by a proteolytic enzyme or other chemical agent. Due regard must of course be had for the possible destruction of the toxin by the chemical agents used.

Several different antitoxic preparations were exposed to the action of trypsin-sodium carbonate solution or to pepsin-hydrochloric acid for comparatively long periods of time, with suitable controls. At proper intervals the extent to which digestion had taken place was measured chemically and compared with the loss of antitoxic units; the latter were determined by guinea pig inoculation as described by Rosenau and Anderson[2]. The results may be summarized as follows:

1. Tetanus antitoxin in 0.5% sodium carbonate solution was slowly and completely destroyed. At the same time no significant chemical changes in the proteins were detected.
2. In solutions amphoteric or faintly acid to litmus strips, trypsin destroys the antitoxin and at the same time the associated proteins are digested. The rates of antitoxin destruction and protein splitting were substantially the same.

From (1) it follows that antitoxin destruction may take place with or without protein splitting.

3. In solutions containing trypsin and 0.5% sodium carbonate the results were the same as in (2).

4. Tetanus antitoxin in 0.2% hydrochloric acid was completely destroyed in three or more days. During this time no significant chemical changes in the proteins were detected.

5. In neutral solutions pepsin did not affect the antitoxin.

6. In pepsin-hydrochloric acid, proteolysis and antitoxin destruction proceeded simultaneously.

These results tend to indicate that tetanus antitoxin is a substance of non-protein nature. But the stability of the antitoxin is so dependent upon that of the protein to which it is attached, that whenever the protein molecule is split, the antitoxin splits with it.

The experimental details are given in the *Journal of Agricultural Research*, 1918.

[1] Homer, A., *J. Hygiene, London*, **15**, 1916, (388–400).

[2] Rosenau, M. J., and Anderson, J. F., *Hygienic Lab. Bull.*, No. 43, 1908.

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# *TESTS FOR FLUORINE AND TIN IN METEORITES WITH NOTES ON MASKELYNITE AND THE EFFECT OF DRY HEAT ON METEORIC STONES*

By George P. Merrill

Department of Geology, United States National Museum, Washington

Communicated by E. W. Morley, April 29, 1918

The following is a partial report on results obtained in continuation of work under a grant from the J. Lawrence Smith Fund of the National Academy of Sciences.

*1. On Fluorine in Meteoric Stones.*—So far as I am aware the occurrence of fluorine has never been recognized in meteoric stones. Meunier[1] records its presence as doubtful. Fletcher in his Handbook does not mention it at all, nor is it mentioned by Cohen nor by Lockyer in his Meteoritic Hypothesis as one of the elements even recognizable by the spectroscope. Nevertheless, the occurrence of a calcium phosphate has often suggested its possible presence, and the wide distribution of this phosphate in meteoric stones, which I have shown of late[2] seemed to warrant further tests, particularly as new and more refined methods for its detection had been discovered. Opportunity for these tests was recently afforded by Dr. E. T. Wherry when engaged upon the investigation of some fossil bones for Dr. Hrdlička in the Museum laboratories. The method consists in the digestion of the material in concentrated sulphuric acid in a small flask heated to 200°C. in a paraffine bath, the process being continued for four to five hours. The gases evolved are bubbled through water, the fluorine being retained in water solution (in a U tube). In ordinary work it is customary to titrate the solution thus obtained with a standard alkali, but inasmuch as the meteoric samples tested contained both chlorides and sulphides, which would yield hydrochloric acid and sulphur dioxide, it was necessary to make the solution first alkaline with sodium hydroxide and evaporate nearly to dryness in a platinum dish on the water bath, the resulting concentrated solution being then added to a standard peroxidized titanium solution in a colorimeter. Fluorine has the power of decolorizing or at least reducing the intense yellow color of this titanium solution, even when present to an amount not exceeding 0.001%.

Three samples were tested—Bluff, Texas; Allegan, Michigan; and Waconda, Kansas, in each of which phosphoric acid ($P_2O_5$) to the amount of 0.25% has been recorded, and in all of which the phosphate had been recognized microscopically. Amounts of from 10 to 20 grams were used in the tests, and in not a single instance did the titanium solution show the least sign of the presence of fluorine. It would seem safe to assume, then, that in these cases at least the element was not present.

2. *Further Tests for Tin in Meteorites.*—It will be recalled that in my report on previous investigations, I stated that no traces of tin had thus far been found by us. Incited, however, by the work of Derby[3] I was led to follow up the matter still further. Derby, it will be remembered, reported 1.18% tin in the schreibersite of the Cañon Diablo iron. Concerning this, he states, "Tin has not been reported (i.e., previously) possibly because the solution has usually been made in aqua regia in which it would only appear through a special research. In the present case, the solution was made in plain nitric acid and the tin appeared as oxide and was verified by blowpipe tests."

Having obtained a considerable quantity of material, chiefly schreibersite and cohenite with some carbon from various digestions of the Cañon Diablo iron in dilute hydrochloric acid, I submitted it to Dr. Whitfield with the request that he examine the same with no other end in view than the determining of tin, if present. The results were negative. Two lots of 5 grams each were taken and dissolved in nitric acid as described by Derby. Not a trace of tin could be found, either in the first solution, as oxide, or by treating the solution with hydrogen sulphide, in the customary way.

On the assumption that there is no error in Derby's work, we must assume as suggested by him, that the tin does not belong to the schreibersite but to another mineral that is not generally distributed throughout the meteoric mass so that it only appears in certain portions of the residue.

3. *On Maskelynite.*—In a very large proportion of the stony meteorites I have described, or studied, mention is made of a colorless, interstitial material either quite isotropic or slightly doubly refracting, with rather low index of refraction, which, following Tschermak and others, I have called, though

often with mental reservations, maskelynite. The material first described under this name, it will be remembered, was found as a prominent constituent of the meteorite of Shergotty, and was sufficiently abundant to allow a satisfactory determination of all of its properties, including chemical composition. All occurrences since noted are of microscopic dimensions—mere interstitial areas of rarely more than two or three millimeters in diameter, and determinable properties so nearly negative that the referring of the mineral to maskelynite has been more in the nature of an acknowledgment to the quality of Tschermak's work than to actual determinations on the part of those describing it. This certainly was true in my own case,[4] and it was not until I was studying the stone of Holbrook, Arizona (1912), that I separated particles and by the recently introduced immersion method determined the index of refraction to be 1.51, which, according to Larsen's tables, is that of an oligoclase glass. Since that writing I have followed up the matter as systematically and thoroughly as time and opportunity will permit, and have reached the conclusion, pronounced without hesitation, that the mineral is in all cases feldspathic, ranging in composition from oligoclase to anorthite, and owes its condition to a fusion since the original crystallization of the stone, followed by a cooling too rapid to allow it to regain its normal properties.

These conclusions are based upon examinations of a large number of sections in which I have found the mineral in all stages from a glass essentially isotropic with the low index (1.51) mentioned above to one plainly biaxial but without crystal outlines, cleavage, or other recognizable properties, with indices of 1.543 and 1.545, and in one case (Ness Co.) 1.56. Also in forms where the mineral is largely isotropic but still retains, in places, traces of plagioclase twinning. It is this last feature, it should be stated, that causes me to consider it a re-fused feldspar, rather than a residual and original feldspathic glass.

These observations, it will be observed, are supplemental and corroborative of those of Tschermak.[5] The subject seems worthy of this extended notice, not merely on account of the new observations, but since Farrington in his recent *Meteorites* remarks concerning the mineral that "Its exact nature is yet to be determined."

Attention should be called, in this connection, that an elevation of temperature sufficient to fuse a feldspar without at least partial destruction of the olivine would be impossible but in an atmosphere completely devoid of all oxidizing gases. (See further under Effects of Heating, below.)

4. *Effects of Heating Meteoric Stones at Various Temperatures.*—The fact that Meunier had transformed meteoric stones of his aumalite group into tadjerites, by heating to redness, suggested the availing myself of opportunities offered by the Pennsylvania Zinc Company at their works at Palmerston, Pennsylvania. A series of prepared specimens, including two cubes, one each of the Estacado and Homestead meteorites, some 10 mm. in di-

ameter, were introduced into pits bored in fire-brick, and sealed up with fire-clay. These bricks were then placed on top of a gas-fucl zienc smelter, where they were allowed to remain for a period of four months, and in an atmosphere in which oxidizing influences were reduced to a minimum. The exact temperature could not be determined, but a cube of the Casas Grandes iron in one of the pits was completely fused and absorbed into one of the bricks, indicating a temperature not less than 1450°C. The results on the two stones were as follows:

*Estacado, Texas.*—This, a veined crystalline chondrite of a dark gray color, fine and compact texture, consists essentially of olivine and enstatite with smaller amounts of pyrrhotite and nickel-iron. Before heating, the silicates are colorless and limpid, and the metallic constituents scattered in small granules fairly uniformly throughout the mass. The roasting resulted in producing a slight glaze on the exterior surface of the cube. The color was much darkened, becoming uniformly dull black. Although remaining firm and hard, the stone became filled with fine vesicles. The thin section under the microscope seemed at first completely amorphous. In strong sunlight, however, the silicates, although nearly opaque and without action in polarized light, were of a deep dull red, indicating a certain amount of oxidation of the iron. The interstices were filled with a fine, dust-like, opaque and amorphous matter which is impossible of determination. The particles of metal had been fused and the material diffused throughout the ground to appear in the form of minute blue points in reflected light. With the exception of the metal not a single one of the original constituents was recognizable.

*Homestead, Iowa.*—This is a dark gray, homogeneous, hard and firm stone belonging to Brezina's brecciated gray chondrite group. The mineral composition, as determined by Wadsworth, is olivine, enstatite, pyrrhotite, iron, and base. Lasaulx is quoted as having noted the occurrence of a feldspar. I find, in addition, a polysynthetically twinned pyroxene and a calcium phosphate in small quantities. After the roasting, the color is dull black and the texture finely vesicular, although firm and hard. As was the case with the Estacado stone, it is so opaque in thin sections as to almost entirely obscure its original structure. The silicates are altered in the same manner, though the olivine is the most affected; the metal is likewise diffused, and attempts at making a photo-micrograph from the slides resulted in complete failure. As it was evident the experiment had been carried too far a second attempt was made in the museum laboratory at lower temperatures and for shorter periods.

Small pieces of the Homestead meteorite, of 2 or 3 grams weight, were roasted in a covered crucible at a low red heat for periods of one-half and one hour each, the gas flame playing freely up and around the crucible. The external manifestations of this heating were a change in color to dark gray and a fusion, on the outer surface, of the troilite granules. A thin section showed the nickel-iron to be unchanged, though the troilite had been broken up to a greater or less extent. The principal change lay in the finer siliceous material occupying the interstices of the other minerals which had turned, in part, black and amorphous. A like change had taken place along the borders of the chondrules and cleavage and fracture lines of the larger phenocryst, giving them the appearance of having been injected with some dark coloring fluid. The amount of change was proportional to the length of time in heating, as shown in figures 1, 2 and 3.

Figure 1 is from the unaltered stone and 2 and 3 from fragments heated for one half and one hour respectively. It will be noted that there is an increase in the amount of black opaque matter in 2 and 3 over that in 1. In the piece roasted for a full hour the fine interstitial silicates have become wholly changed to the black matter which penetrates the borders of the chondrules and other crystal aggregates, until a condition is reached so closely resembling that shown in sections of the McKinney (fig. 4) and Travis County (fig. 5) black chondrites as to apparently leave no doubt as to the correctness of Meunier's view to the effect that such are but phases of chondritic stones which have been altered through a re-heating subsequent to their first crystallization. It should be added that these roasted pieces are partially restored to their original color by digestion in hydrochloric acid and sodium carbonate, showing that the change is one that has influenced chiefly the olivine. Incidentally attention may well be called to the manner in which the blackening of the stone first manifests itself along cleavage and fracture lines in figure 5.

It should be noted further that it is doubtful if all of the dark color in these black chondrules is due to roasting, since some of them heated in a closed tube give evidence of the presence of a small amount of a hydrocarbon.

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[1] *Encyclopédie Chimique*, **2**, appendix 2, Meteorites.

[2] *Amer. J. Sci., New Haven*, **43**, 1918, (322).

[3] *Ibid.*, **49**, 1895, (101–110).

[4] See my papers on the meteorites of Holbrook, Arizona, *Smithsonian Misc. Coll., Washington*, **60**, No. 9, 1912; Modoc, Kansas, *Amer. J. Sci., New Haven*, **21**, 1906, (356–360); Rich Mountain, North Carolina, *Proc. U. S. Nat. Mus., Washington*, **32**, 1907, (241–244); Thomson, Georgia, *Smithsonian Misc. Coll.*, **52**, 1909, (473–476); Fisher, Minnesota, *Proc. U. S. Nat. Mus.*, **48**, 1915, (503–506); and Coon Butte, Arizona, (Mallet), *Amer. J. Sci.*, **21**, 1906, (351).

[5] See Cohen's *Meteoritenkunde*, pp. 311–314, Figure 2 on plate 17 or Tschermak's *Die Mikroskopische Beschaffenheit der Meteoriten* will apply equally well to the majority of occurrences.

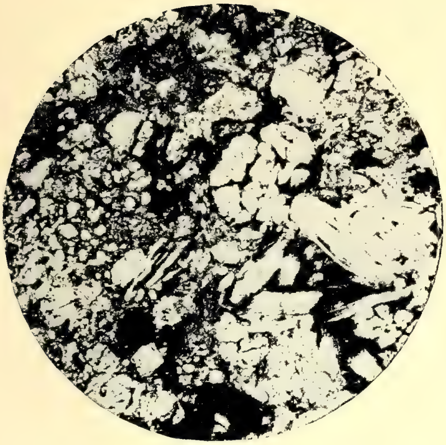

FIG. 1

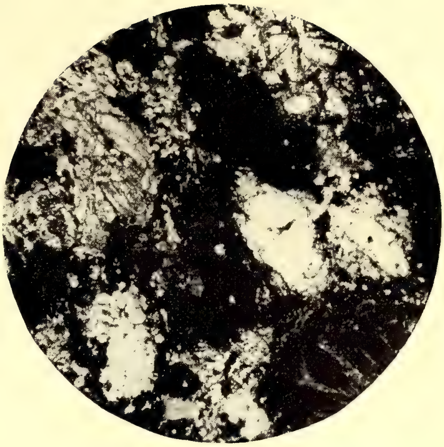

FIG. 2

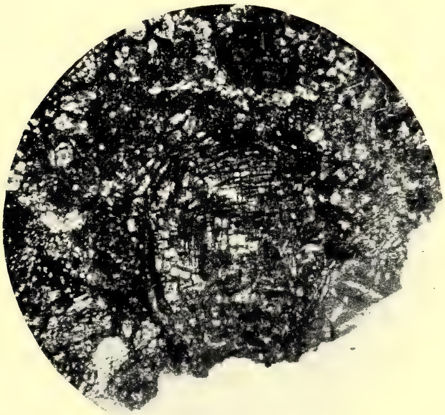

FIG. 5

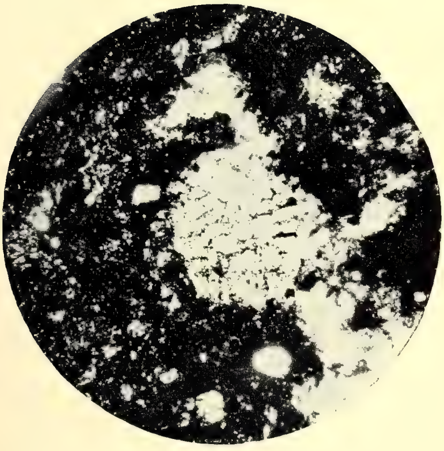

FIG. 3

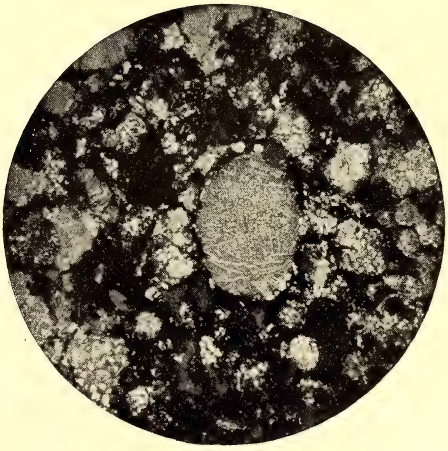

FIG. 4

# NOTES ON ISOTOPIC LEAD

By Frank Wigglesworth Clarke

United States Geological Survey, Washington

Read before the Academy, April 23, 1918

One of the most remarkable discoveries in the field of radioactivity, has been the fact that the elements of highest atomic weight, uranium and thorium, are unstable, and undergo slow transformations into other substances; especially into helium and lead. The lead thus produced is identical with normal lead in its spectrum and its distinctively chemical properties, but different in its atomic weight; and this difference, which is thoroughly established, is of peculiar significance. The purest lead from uranium minerals has an atomic weight fully a unit lower than that of ordinary lead, while that from thorium minerals is nearly a unit higher. These are the extreme differences, so far as the present evidence goes; but the actual determinations of the atomic weights of these *isotopes* of lead show wide variations due to differences between the minerals from which the lead was obtained. Furthermore, these isotopes differ from ordinary lead in specific gravity; one being lighter and the other heavier than ordinary lead, these differences being proportional to the variations in atomic weight. Consequently the three kinds of lead have the same atomic volume, and occupy the same place in the periodic classification of the chemical elements.

Ordinary or *normal* lead differs from isotopic lead in one important respect, namely, its atomic weight is constant, and the actual determinations vary only within the limits of experimental uncertainty. This constancy was established by Baxter and Grover,[1] who studied lead from a number of distinct sources. Their material was derived from four mineral species; galena, cerussite, vanadinite, and wulfenite, and also from commercial lead nitrate. Furthermore, the minerals examined came from seven widely separated localities; two from Germany, and one each from Australia, Missouri, Idaho, Washington, and Arizona. The lead in each case was carefully purified, and converted into chloride, with which the determinations of atomic weight were made. The method of determination was the standard method long in use at Harvard, and based upon large experience and the most thorough technique. The values found for the atomic weight are shown in the following table:

| *Source* | *Atomic weight of lead* |
|---|---|
| Commercial nitrate | 207.22 |
| Cerussite, New South Wales | 207.22 |
| Cerussite, Eifel Mountains, Germany | 207.20 |
| Galena, Joplin, Missouri | 207.22 |
| Cerussite, Wallace, Idaho | 207.21 |
| Galena, Nassau, Germany | 207.21 |
| Vanadinite and wulfenite, Arizona | 207.21 |
| Galena, Metalline Falls, Washington | 207.21 |

A series of analyses of lead bromide gave values practically identical with these.

This evidence as to constancy of atomic weight is conclusive, but it has also been confirmed by three investigations by Richards and his colleagues in this country, and by Hönigschmid in Vienna. These later determinations were made as checks upon determinations of the atomic weight of isotopic lead derived from uranium minerals.

In 1914 Richards and Lembert[2] published their determinations of the atomic weight of isotopic lead. Their results may be summarized as follows:

| *Source* | *Atomic weight* |
|---|---|
| Lead from Ceylonese thorianite | 206.82 |
| Lead from English pitchblende | 206.86 |
| Lead from Colorado carnotite | 206.59 |
| Lead from Bohemian pitchblende | 206.57 |
| Lead from North Carolina uraninite | 206.40 |

Two years later another series of determinations by Richards and Wadsworth[3] appeared. The average results obtained were as follows:

| *Source* | *Atomic weight* |
|---|---|
| Australian carnotite | 206.375 |
| Colorado carnotite | 207.004 |
| Bröggerite, Norway | 206.122 |
| Cleveite, Norway | 206.085 |

Still another series of six determinations by Richards and Hall[4] on lead from Australian carnotite gave a mean value for the atomic weight of Pb = 206.415.

In a preliminary study of lead from Bohemian pitchblende, Hönigschmid[5] found values for the atomic weight ranging from 206.719 to 206.749. In a later investigation by Hönigschmid and Horovitz,[6] lead was extracted from three different minerals, namely, the purest Joachimsthal pitchblende, a crystallized uranium ore from Morogoro, German (?) East Africa, and bröggerite from Norway. The average values for the atomic weight were as follows:

| *Source* | *Atomic weight* |
|---|---|
| Pitchblende | 206.406 |
| The Morogoro ore | 206.042 |
| Bröggerite | 206.067 |

All of these determinations of atomic weight, including those of Baxter and Gover on normal lead, were made by the same method, the same care as to purity of materials, and the same refinements of technique. Even Hönigschmid, now in Vienna, had worked on atomic weight determinations with Richards, and so was familiar with the best procedure. The results obtained are therefore strictly comparable.

For the atomic weight of thorium lead the data as yet are scanty, and based entirely upon material derived from Ceylonese thorite and thorianite.

From the specific gravity of thorite lead Soddy[7] has deduced the atomic weight of Pb = 207.64; and Hönigschmid[8] from analyses of lead chloride prepared from Soddy's original material has found Pb = 207.77. This value, however, is probably too low for the true thorium lead, for the reason that thorite, with a preponderant proportion of thoria, also contains some uranium. The thorite lead, therefore, must contain both isotopes, but with the higher one in much the largest quantity. The thorianite lead studied by Richards and Lembert had a still lower atomic weight, namely Pb = 206.82, which shows that this variety of the metal is not of uniform character.

That the atomic weight of uranium lead is extremely variable has already been shown. In order to interpret this variability its sources must be studied both geologically and mineralogically. On the geologic side of the question the uranium ore can be divided in to three principal classes, which are sharply distinct. The definitely crystallized varieties of uraninite occur in coarse pegmatites, associated with feldspar, quartz, mica, beryl, and other minor accessories. The massive pitchblende is found in metalliferous veins, together with sulphide ores of copper, lead, iron, zinc, and so forth. As for carnotite, that is a secondary mineral, found commonly as an incrustation on sandstone, and often, also upon fossil wood. There may be other modes of occurrence, but these are the most distinctive.

In chemical composition the uraninites, as shown by Hillebrand's[9] splendid series of twenty-one analyses, fall into well defined groups. All contain uranium oxides, ranging from 65 to 90%, the low figures, however, representing altered material. The crystallized, pegmatitic uraninites are characterized by their content in thoria and other rare earths, from 6 or 7 up to as much as 11%. They also contain subordinate proportions of lead, and the largest amount of helium. In bröggerite and cleveite, however, lead is in excess of thoria. The massive pitchblendes, on the other hand, contain no thoria, usually much lead and little or no helium. That from Black Hawk, Colorado, is exceptional. It is intimately associated with sulphide ores, but contains little lead, and zirconia instead of thoria. Carnotite, which is quite unlike uraninite, is essentially a vanadate of uranium and potassium, with very little lead and no helium. It is, however, an important source of radium.

It is now possible to correlate, at least roughly, the composition of the several minerals with the determinations of the atomic weight of uranium lead, although for a perfect comparison we should have analyses of the actual ores from which the various samples of lead were obtained. On theoretical grounds it is supposed that the true atomic weight of uranium lead is not far from 206, and only determinations which approach that value are those which represent crystallized uraninite, including the varieties bröggerite and cleveite. These minerals all contain helium, so that there seems to be a relation between the formation of these two degradation products of uranium. The minerals also contain thorium, which would tend to raise the atomic weight and so complicate any discussion of the figures. The most brilliantly crystallized uraninite, that from Branchville, Connecticut, contains 85% of $UO_3 + UO_2$, with about 7% of $ThO_2$, 4.35% of PbO, and a maximum, 0.4% of helium. The atomic weight of lead from that source, unfortunately, has not been determined; and it is doubtful whether material enough for accurate investigation could be obtained.

The other determinations of the atomic weight of uranium lead give values much above 206, and even approaching 207. This is especially true of the lead from pitchblende, which contains no thorium and little if any helium. Its association with sulphide ores, however, leads to the suspicion that it may contain ordinary lead, perhaps in the form of occluded or dissolved galena. The atomic weight of the lead derived from it would, therefore, be that of a mixture, and not of the isotope alone. The carnotite lead would also seem to be a mixture, but of what kind is not clear.

The atomic weight of isotopic lead now seems to be a complex of at least three quantities, namely, the atomic weights of normal lead, uranium lead, and thorium lead, in varying proportions. Since the atomic weights of the two isotopes differ from that of normal lead in opposite directions it is difficult to determine in any particular case the relative proportions of the three modifications of the element. It has been suggested that normal lead is a balanced mixture of its isotopes; but the constancy of the atomic weight of the ordinary metal seems to negative that supposition. In order to fulfill this condition it would be necessary that the isotopes should always commingle in equal or at least definite proportions; which is extremely improbable. The apparent variations in the atomic weight of lead, as shown in the older determinations, are due to varying methods, imperfect technique, different values for the atomic weights of the other elements with which that of lead is compared, and experimental errors. The modern determinations, which I have already cited, are the only ones that are strictly comparable.

The suggestion that the lead contained in uranium ores is partly normal lead is not new. It has been advanced by other writers,[10] but the variable atomic weight of uranium lead gives the supposition a decided emphasis. It now acquires new importance because of its bearing upon certain attempts to use the ratio between uranium and lead in uranium minerals as a datum for computing the age of the earth. For this purpose the ratio has been employed by Boltwood,[11] who calculated it from almost all the trustworthy analyses of uraninite and its nearly allied species, and from it deduced their ages. These ages differ exceedingly. For a crystallized uraninite from Connecticut he found the age to be 410,000,000 years, and for Ceylonese thorianite 2,200,000,000 years. These calculations, and others, like them, involve two assumptions; first, that the rate of change from uranium to lead is accurately known, and secondly that all the lead was of radioactive origin.

The latter assumption is now seen to be extremely doubtful for the varying atomic weights prove that more than one kind of lead must be considered. Thorium lead especially must be taken into account, for many uraninites contain it, and in thorianite the percentage of thoria is more than five times that of uranium oxide. The ratio of lead to its parent elements is therefore much less than Boltwood assumed, and the calculated age of thorianite is vastly reduced. Boltwood, however, doubted the derivation of lead from thorium, a fact which was not definitely known at the time his paper was written. The evidence of the atomic weights is also much later.

Furthermore, the doubtful applicability of Boltwood's method to chronological measurements has been shown by G. F. Becker;[12] who applied it to the analyses of rare-earth minerals from one locality in Llano County, Texas. The figures given by Becker are as follows:

| *Mineral* | *Analyst* | *Calculated age in years* |
|---|---|---|
| Yttrialite | Mackintosh | 11,470,000,000 |
| Yttrialite | Hillebrand | 5,136,000,000 |
| Mackintoshite | Hillebrand | 3,894,000,000 |
| Nivenite | Mackintosh | 1,671,000,000 |
| Fergusonite | Mackintosh | 10,350,000,000 |
| Fergusonite | Mackintosh | 2,967,000,000 |

These ages differ enormously, even between two analyses of the same mineral. This evidence, taken together with the evidence from the atomic weights, seems clearly to show that the uranium-lead ratio is not applicable to the determination of the age of minerals. It is quite certain that not all of the lead in uranium ores is of radioactive origin. In pitchblende, for example, which contains no thorium, the determinations of atomic weight range from 206.40 to 206.88, figures far in excess of the theoretical 206.00 which is assigned to pure uranium lead. Normal lead, perhaps in solid solution, must be present in such ores.

What, now, is the fundamental difference between normal lead and isotopic lead? The answer to that question must be largely speculative; but speculation is legitimate when its purpose is to stimulate future research. One difference at least may reasonably be assumed, namely, that normal lead is the product of an orderly evolution of the chemical elements; and that isotopic lead is a product of their decay. Creation is one process, destruction is the other.

Forty-five years ago[13] I ventured to suggest that an evolution of the elements had actually occurred. It was clearly indicated by the progressive chemical complexity of the heavenly bodies, from the chemically simple gaseous nebulae, through the hotter stars and the sun, to the finished planets like our earth. At first, hydrogen and helium were the most abundant and conspicuous elements, then elements of higher atomic weight gradually appeared, and at the end of the process there was the chemical complexity of the earth, in which the free elements had in great part been absorbed and replaced by a multitude of compounds. On this basis hydrogen and helium seem to be the oldest of the known elements, while uranium and thorium are the youngest of all. Lead is older than uranium and thorium, for its lines appear in the solar spectrum, in which the other two elements have not as yet been recognized. Lead, however, is vastly more abundant than either uranium or thorium, and is more likely to have been originally their progenitor than their child.

Up to this point we have a reasonable interpretation of definite evidence, beyond this, imagination must come into play. It is fair to assume that the process of evolution was extremely slow, and that each element was developed gradually and passed from an unfinished to a finished stage. The chemical atoms are now known to be extremely complex structures, each with an electropositive nucleus surrounded by electrons in rapid motion. That such a structure could have been developed instantaneously, with no previous preparation, is hardly probable, for the process was one of condensation, from lighter to heavier, and that, it would seem, must have acquired time. The process was one from relative simplicity of structure to relative complexity, and with the maximum condensation, as shown by uranium and thorium, a minimum of stability was reached. That is, so far as we now know; for less stable atoms may have been formed, to exist for a brief period and then vanish. Some of the radioactive elements which appear as products of the decay of uranium are of this kind. On that theme, more later.

That the atoms of the elements above helium in the scale of atomic weights could not have been formed instantaneously is indicated by their structure. It has been shown that they are built up of smaller particles, of electrons, and also in part, perhaps, of preexistent helium. Such particles, approaching one another, at first in irregular proportions, are supposed to have formed the atoms in question; but that exactly the right proportions for stability were found at once is hardly conceivable. There must have been a period of selection, in which the unavailable particles were discarded, probably to be used in other structures later. For each new chemical atom a definite balance between electropositive and electronegative particles was required, and also the establishment of a stable configuration. When these conditions were fulfilled the atom of an element was complete. As I have already said we can fairly assume that there was a distinct passage from an unfinished or incipient structure to a finished one of permanent stability. Furthermore, as shown by the spectra of stars and nebulae, the elements of relatively low atomic weight were first formed, and those of higher atomic weight came later. The older elements were also developed in the largest quantities, and are therefore the most abundant. The later elements are as a broad general rule much scarcer. This rule is not absolutely exact, but it expresses some well known general relations. The very simple and very stable primordial helium, however, is now relatively rare; but there is evidence to make us believe that it was largely consumed in building other elements. Its present observed emission by radium is evidence in favor of this supposition.

In this evolutionary hypothesis with its subsidiary speculations there is, I think, nothing incompatible with present knowledge. In matters of detail it is unavoidably incomplete; but notwithstanding its imperfections it bears very directly upon a consideration of the later phenomena of radioactive decay. Here the process of evolution is reversed and rapid changes take the place of slow ones. Furthermore, the normal elements are supposed to be veritable store-houses of potential energy; which, in radioactive changes becomes partly kinetic. Radium, for example, gives forth heat continuously; and its rate of decay can be observed in the laboratory.

Through the investigation of radioactive transformations more than thirty new substances, elements or pseudo-elements, have been discovered. Some of these are extremely evanescent, lasting only for seconds or even fractions of a second; others are relatively long lived. All of them, however, are more or less unstable, and change, slowly or swiftly, into other things. Some of them are metallic, like radium, polonium, and actinium; others appear as emanations which belong to the group of the chemically inert gases. One of these, helium, is continuously being generated from radium. Some, again, are isotopic with bismuth or thallium; and four of them are said to be isotopes of lead. These are Radium B, Thorium B, Actinium B, and Radium D. The first three are short lived, and endure only for a few minutes or hours, but Radium D, also known as radio-lead, is assigned a probable life period of 24 years, and given theoretically an atomic weight not far from 210. In its chemical relations it cannot be distinguished from lead.

All four of these isotopes may have been present in uranium lead at the time of its formation, but it does not seem possible that even a trace of them could persist in the lead which is now extracted from uraninite or thorianite. They are therefore negligible in our consideration of the evidence which is now supplied by the study of the atomic weights, except in so far as they show the probable derivation of uranium lead and thorium lead from the two higher elements. The essential point is that all these varieties of lead are products of degradation, and in that respect differ fundamentally from the normal product of evolution. The thirty or more new substances which have been revealed to us by the study of radioactivity are all matter in a a state of transition from instability towards some stable form, which may be lead, or bismuth, or thallium, or some other element which has not yet been recognized as an end product of these mysterious changes. As these products are approached we have them in an incomplete condition, nearly but not quite identical with the permanent elements: This may be the character of isotopic lead. The fact that uranium lead is radioactive shows that it is still undergoing change; and that its atoms have not acquired the exact composition and configuration which give to normal lead its uniformity and stability. Whether or not the process of change can continue until normal lead is formed, it is impossible for us to say.

The atoms of the chemical elements, are, as I have already said, extremely complex, but their structure is not yet completely understood. To some part of each kind of atom its chemical properties and its spectrum are probably due. It is conceivable that this part may be the earliest to form, with its surrounding rings or envelopes at first not quite adjusted to permanent stability. With the final adjustment the isotopes as such should disappear, and the normal element be completed. This is speculation, and its legitimacy remains to be established. A careful comparison of the spectra of the elements from thallium up to uranium might furnish some evidence as to its validity. The spectrum of uranium, for example, may contain lines which really belong to some of its derivatives.

*Note.*—Since this paper was written, one by Professor Barrell[14] has appeared, in which the use of the uranium-lead ratio for determining the age of minerals is defended. There are also two papers by Holmes and Lawson,[15] and another by Holmes,[16] in which the same position is taken. There is evidently room for further discussion of the subject, but as yet I see no good reason to change my own views.

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[1] *J. Amer. Chem. Soc., Easton, Pa.*, **37**, 1915, (1027).

[2] *Ibid.*, **36**, 1914, (1329).

[3] *Ibid.*, **38**, 1916, (2613).

[4] *Ibid.*, **39**, 1917, (531).

[5] *Zsch. Elektrochem., Halle*, **20**, 1914, (457).

[6] *Monatsh. Chem., Vienna*, **36**, 1915, (355).

[7] *Nature, London*, **94**, 1915, (615).

[8] *Chem. Abst.*, **11**, 1917, (3173). From *Physik. Zs.*, **18**, 1917, (114).

[9] *Washington, U. S. Geol. Survey, Bulls.* 78 and 90. Also in *Bull.* 591, pp. 366–368. Hillebrand discusses the mode of occurrence of these minerals, much as I have done.

[10] See Joly, *Phil. Mag.*, (6), **22**, 1911, (354). and Becker, *Bull. Geol. Soc. Amer.*, **19**, 1908, (134).

[11] *Amer. J. Sci., New Haven*, (4), **23**, 1907, (86).

[12] *Bull. Geol. Soc. Amer.*, (4), **19**, 1908, (134). See also Zambonini, *Rome, Atti Acc. Lincei* (5), **20**, part 2, 1911, (131).

[13] *Popular Science Monthly*, January, 1873.

[14] *Bull. Geol. Soc. Amer.*, **28**, 1918, (745).

[15] *Phil. Mag.*, (6), **28**, 1914, (823), **29**, 1915, (682).

[16] *Proc. Geologist's Assoc., London*, **26**, 1915, (289).

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# *ON THE REPRESENTATION OF A NUMBER AS THE SUM OF ANY NUMBER OF SQUARES, AND IN PARTICULAR OF FIVE OR SEVEN*

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Communicated by E. H. Moore, May 21, 1918

1. The formulae concerning the representation of a number as the sum of 5 or 7 squares belong to one of the most unfamiliar and difficult chapters in the Theory of Numbers, and only one proof of them has been given. The proof depends on the general arithmetic theory of quadratic forms, initiated by Eisenstein and perfected by Smith and Minkowski. This theory, of which a systematic account will be found in the fourth volume of Bachmann's *Zahlentheorie* gives a complete solution of the problem of any number $s$ of squares not exceeding 8. Beyond $s = 8$ it fails.

When $s$ is *even* there is an alternative method. This method, which depends on the theory of the elliptic modular functions, is much simpler in idea than the method of Smith and Minkowski; and it has another very important merit, that it can be used—within the limits of human capacity for calculation—for *any* even value of $s$. Thus Jacobi solved the problem for 2, 4, 6 and 8. In these cases the number of representations can be expressed in terms of the divisors of $n$. Suppose, *e.g.*, that $s = 8$; and let us write, generally,

$$1 + \sum_{1}^{\infty} r_s(n) q^n = (1 + 2q + 2q^4 + \ . \ . \ . \ )^s = \{\vartheta_3(0, \tau)\}^s = \vartheta^s,$$

where $q = e^{\pi i \tau}$. Then

$$\vartheta^8 = 1 + 16\left(\frac{1^3 q}{1+q} + \frac{2^3 q^2}{1-q^2} + \frac{3^3 q^3}{1+q^3} + \frac{4^3 q^4}{1-q^4} + \ . \ . \ . \ \right),$$

and $r_8(n)$ is $16 \Sigma \delta^3$ if $n$ is odd and $8 \Sigma \delta_0^3 - 8 \Sigma \delta_1^3$ if $n$ is even, $\delta$ denoting a divisor of $n$, $\delta_0$ a even, and $\delta_1$ an odd divisor. When $s$ exceeds 8 the formulae are less simple, and involve arithmetical functions of a more recondite nature. Liouville gave formulae concerning the cases $s = 10$ and $s = 12$, and Glaisher[1] has worked out systematically all cases up to $s = 18$. More recently important papers on the subject, to which I shall refer later, have been published by Ramanujan[2] and Mordell.[3] In the latter paper the whole subject is exhibited as a corollary of the general theory of modular invariants.

The primary object of my own researches has been *to deduce the formulae for $s = 5$ and $s = 7$ from the theory of elliptic functions*, and so to place the cases in which $s$ is odd and even, so far as may be, on the same footing. The methods which I use have further important applications, but this is the one which I wish to emphasize at the moment. The formulae which I take as my goal are the formulae

$$r_5(n) = \frac{Bn\sqrt{n}}{\pi^2} \sum \left(\frac{n}{m}\right)\frac{1}{m^2}, \tag{1}$$

$$r_7(n) = \frac{Cn^2\sqrt{n}}{\pi^3} \sum \left(\frac{-n}{m}\right)\frac{1}{m^3}, \tag{2}$$

given by Bachmann (pp. 621, 655). Here $n$ as an odd number not divisible by any square (so that there is no distinction between primitive and imprimitive representations); $m$ runs through all odd numbers prime to $n$; B is 80, 160, 112, or 160, according as $n$ is congruent to 1, 3, 5 or 7 (mod. 8); and C is 448, 560, 448 or 592 in similar circumstances. These formulae are the central formulae of the theory: they involve infinite series, but these series are readily summed in finite terms by the methods of Dirichlet and Cauchy. With them should be associated the formula

$$r_3(n) = \frac{A\sqrt{n}}{\pi} \sum \left(\frac{-n}{m}\right)\frac{1}{m}, \tag{3}$$

where A is 24, 16, 24, or 0: but this formula, as we shall see, stands in some ways on a different footing.

2. My new proof of the formulae (1) and (2) was arrived at incidentally in the course of researches undertaken with a different end, that of finding *asymptotic* formulae (valid for *all* values of $s$) for $r_s(n)$ and other arithmetical functions which present themselves as coefficients in the expansions of elliptic modular functions. In a paper[4] shortly to appear in the *Proceedings* of the London Mathematical Society, Mr. Ramanujan and I have developed an exceedingly powerful method for the solution of problems of this character, and applied it to the study of $p(n)$, the number of (unrestricted) partitions of $n$. This method, when applied to our present problem, introduces the function

$$\Theta_s(q) = 1 + \frac{\pi^{\frac{1}{2}s}}{\Gamma(\frac{1}{2}s)} \sum_{h,k} \left(\frac{S_{h,k}}{k}\right)^s F(qe^{-h\pi i/k}), \tag{4}$$

where

$$S_{h,k} = \sum_{p=0}^{k=1} e^{p^2 h\pi i/k}, \quad F(q) = \sum_{1}^{\infty} n^{\frac{1}{2}s-1} q^n,$$

and the summation applies to $k = 1, 2, 3, \ldots$, and all positive values of $h$ less than, of opposite parity to, and prime to $k$ ($h = 0$ being associated with $k = 1$ alone). The coefficient of $q^n$ in $\Theta_s(q)$ is

$$\chi_s(n) = \frac{\pi^{\frac{1}{2}s}}{\Gamma(\frac{1}{2}s)} n^{\frac{1}{2}s-1} \sum_{h,k} \left(\frac{S_{h,k}}{k}\right)^s e^{-nh\pi i/k}; \tag{5}$$

and our method leads to the conclusion that

$$r_s(n) = \chi_s(n) + O(n^{\frac{1}{4}s}), \tag{6}$$

at any rate for every value of $s$ exceeding 4.

When $s$ is *even*, $F(q)$ is an elementary function; and $(S_{h,k})^s$ is easily expressible in a form which does not involve the 'Legendre-Jacobi symbol' $\left(\frac{a}{b}\right)$. The function $X_s(n)$ is then susceptible of a variety of elementary transformations and it was shown by Ramanujan, in the second of his two papers quoted above, that $X_s(n)$ is *identical with* $r_s(n)$ when $s = 4$, 6 or 8. In what follows I confine myself to the case in which $s$ is *odd*, merely remarking that my method (which is entirely unlike that used by Ramanujan) leads directly to an alternative proof of his results.

3. When $s$ is odd, $F(q)$ is not an elementary function. But it is not difficult to prove that

$$\frac{\pi^{\frac{1}{2}s}}{\Gamma(\frac{1}{2}s)} F(q) = \sum_{-\infty}^{\infty} \frac{1}{[(2n-\tau)i]^{\frac{1}{2}s}}, \tag{7}$$

every term on the right hand side having an argument numerically less than $\frac{1}{4}s\pi$. Further, $S^s_{h,k} = S^{s-1}_{h,k}\, S_{h,k}$; and the first factor can always be expressed in a simple form. Suppose, to fix our ideas, that $s = 5$. Then $S^4_{h,k} = (-1)^h k^2$. Substituting from this equation and from (7) into (4), and effecting some obvious simplifications, we obtain

$$\Theta_5(q) = 1 + \sum_{h,k} \frac{(-1)^h S_{h,k}}{\sqrt{k}} \frac{1}{[(h-k\tau)i]^{\frac{5}{2}}} \tag{8}$$

where now $h$ assumes all values of opposite parity to and prime to $k$. This formula may be simplified further by multiplying each side by

$$\frac{1}{1^2} + \frac{1}{3^2} + \frac{1}{5^2} + \dots = \frac{\pi^2}{8}.$$

We then find

$$\Theta_5\,(q) = \frac{8}{\pi^2} \sum_{h,\,k} \frac{(-1)^h\, S_{h,\,k}}{\sqrt{k}} \frac{1}{[(h - k\tau)\, i]^{\frac{5}{2}}}, \tag{9}$$

the summation now extending to $k = 0, 1, 2, \dots$ and *all* $h$ of opposite parity to $k$. This is our fundamental formula, when $s = 5$. Two steps remain: first, to prove the identity of $\Theta_5(q)$ and $\vartheta^5$; secondly, to deduce the formulae of Smith and Minkowski.

4. The first step presents no very serious difficulty, for it involves nothing beyond an adaptation of the ideas used by Mordell in his paper quoted in §1. We prove first that $\Theta_5$ behaves like $\vartheta^5$ in respect to the linear modular transformations $\tau = T + 2$, $\tau = -1/T$; so that $\Theta_5/\vartheta^5$ is an invariant of the modular sub-group called by Klein-Fricke and Mordell $\Gamma_3$. Secondly, by studying the transformation $\tau = (T - 1)/T$, we prove that $\Theta_5/\vartheta^5$ is *bounded* in the 'fundamental polygon' associated with $\Gamma_3$. It then follows that the quotient is a constant which is easily seen to be unity. In all this the only difficulty arises from the use of certain reciprocity-formulae satisfied by Gauss's sums.

We now transform (9) by effecting the summations with respect to $h$, using certain contour integrals of a type common in the work of Lindelöf and other writers. We thus obtain

$$\vartheta^5 = 1 + \frac{32}{3} \left\{ \sum_{1,3,5,\dots} \frac{1}{k^2} \sum_{j} \sum_{m=0}^{\infty} (mk+j)^{\frac{3}{2}}\, q^{mk+j} - \sum_{2,4,6,\dots} \frac{1}{k^2} \sum_{j} \sum_{m=0}^{\infty} (-1)^{m+\mu}\, (mk+j)^{\frac{3}{2}}\, q^{mk+j} \right\} \tag{10}$$

a fundamental identity which contains the whole theory of the representation of numbers by sums of 5 squares. The symbols $j$ and $\mu$ alone require explanation; $j$ runs through the complete set of least positive residues of $0, 1^2, 2^2, \dots, (k-1)^2$ to modulus $k$, each taken as often as it occurs; and $\mu k$ is the multiple of $k$ deducted in order to arrive at such a residue. And the remainder of the work is purely arithmetical. Picking out the coefficient of $q^n$, we obtain a series for $r_5(n)$ which is found, after some reduction, to be equivalent to the series given by Bachmann.

4. The formulae which correspond to (10) for $s = 7$ and $s = 3$ are

$$\vartheta^7 = 1 + \frac{256}{15} \Bigg\} \sum_{1,3,5,\ldots} \frac{(-1)^{\frac{1}{2}(k-1)}}{k^3} \sum_{j} \sum_{m=0}^{\infty} (mk+j)^{\frac{5}{2}} q^{mk+j} \tag{11}$$

$$- \sum_{2,4,6.\ldots} \frac{1}{k^2} \sum_{j} \sum_{m=0}^{\infty} (-1)^{m+\mu} (mk+j)^{\frac{5}{2}} q^{mk+j} \Bigg\{,$$

$$\vartheta^3 = 1 + 8 \Bigg\} \sum_{1,3,5,\ldots} \frac{(-1)^{\frac{1}{2}(k-1)}}{k} \sum_{j} \sum_{m=0}^{\infty} (mk+j)^{\frac{1}{2}} q^{mk+j} \tag{12}$$

$$+ \sum_{2,4,6,\ldots} \frac{1}{k} \sum_{j} \sum_{m=0}^{\infty} (-1)^{m+\mu} (mk+j)^{\frac{1}{2}} q^{mk+j} \Bigg\{.$$

The interpretation of $j$ and $\mu$ is as before, except that, when $k$ is even, $j$ is a residue of one of the numbers $\frac{1}{2}k$, $\frac{1}{2}k + 1^2$, . . . , $\frac{1}{2}k + (k - 1)^2$. These identities embody the theory for 7 or 3 squares. It should be noted however, that the application of my method becomes very much more difficult when $s = 3$, as the double series used are then not absolutely convergent; and in fact the only proof of (12) which I possess consists in an identification of the results which it gives with those already known.

I conclude by a word concerning the cases in which $s > 8$. Here, when $s$ is odd, we are on untrodden ground. We have the asymptotic formula (6); and we can evaluate $X_s(n)$ as when $s = 5$ or 7, thus obtaining a series of new results. But it is no longer to be expected that our results should be *exact*, and I have verified that, when $s = 9$, they are not exact, even when $n = 1$.

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[1] Glaisher, J. W. L., *Proc. London Math. Soc.*, (Ser. 2), **5**, 1907, (479–490).

[2] Ramanujan, S., *Trans. Camb. Phil. Soc.*, **22**, 1916, (159–184); *Ibid.*, (in course of publication).

[3] Mordell, L. J., *Quart. J. Math.*, **48**, 1917, (93–104).

[4] Hardy, G. H., and Ramanujan, S., *Proc. London Math. Soc.*, (Ser. 2), **17**, 1918, (in course of publication).

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# THE CRYSTAL STRUCTURE OF ICE

By Ancel St. John

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During the winter of 1916–1917 the crystal structure of ice was investigated by means of the X-rays. The photographic method originated by deBroglie[1] was used with certain modifications suggested privately by Dr. A. W. Hull. The source of energy was a Coolidge tube with tungsten target excited by an induction coil with mercury turbine interrupter. At first the apparatus already set up for another investigation was used, being kept cool by leaving the laboratory windows open. This method was very uncertain on account of the erratic weather and was otherwise unsatisfactory and was discarded after a single good photograph had been obtained. The spectrometer system was then enclosed and the chamber kept cool by cans of ice and salt. By this means the temperature could be kept reasonably constant but it was found virtually impossible to mount and maintain a specimen long enough to get a satisfactory photograph, probably on account of the presence of the salt vapor. Upon the recommendation of Prof. A. G. Webster a grant was made from the Rumford Fund of the American Academy of Arts and Sciences, Boston, in aid of the investigation which made it possible to install a small ammonia refrigerating machine loaned by the Automatic Refrigerating Company and to build a specially adapted spectrometer mounted in a well-insulated refrigerator box. With this equipment the temperature could be maintained indefinitely and there was no further trouble from melting of specimens. A marked tendency to sublimation, however, was troublesome until each specimen was mounted in a gelatine capsule when equilibrium was quickly established between the crystal and its vapor. Protected in this manner specimens were preserved for days.

Commercial artificial ice was first investigated as it shows marked prismatic structure. Unfortunately the prisms are distorted through pressure in the formation of the ice so that it is difficult to identify cleavage planes. Some photographs show spectral bands but no sharp lines upon which to base calculations. The investigation is to be pursued further, however, as it is probable that sufficiently small crystals or slices of crystals will give sharply defined lines. In a second procedure a thin layer of ice, about 2 mm. thick, was allowed to form on a pan of tap water. It was difficult to identify individual crystals and more difficult to isolate them for mounting but occasionally a reasonably good specimen was secured and mounted so that the axis of rotation bore a definite relation to the original surface of the ice. Orientation with respect to other axes was a matter of guess work, usually wrong as the results showed. Nevertheless several satisfactory photographs were obtained from specimens prepared in this manner, in fact the calculations are based entirely on them. In a third method ice was frozen out of a weak salt solution. In this way a large crop of thin specimens showing distinct cleavage planes could be secured. Owing to the failure of the source of power the investigation was interrupted at this point and has not yet been renewed. One photograph was obtained showing distinct spectral bands but no identifiable lines, possibly on account of the mixture of microscopic salt crystals with the ice. The method is promising and is to be pursued further at a convenient season.

Ice is commonly assigned to the hexagonal system of crystals[2] and is conveniently referred to a triangular space lattice each cell of which has sides

$a$ and a height $c$. An elementary triangular cell of such a structure will have a volume $V = \sqrt{3}a^2c/4$. If the density is $\rho$, the mass of a molecule $m$ and the number of molecules per cell $n$, the mass of the cell is $nm = 3a^2c\rho/4$. For ice[2] $c/a = 1.4026$, $\rho = .91$ gm/cm³., $m = 29.73 \times 10^{-24}$ gm. The molecular weight is taken, as the arrangement of diffracting centers is fundamentally that of the molecule, each pair of hydrogen atoms being presumably near an oxygen atom. These values gave

$$a = (54.35n)^{\frac{1}{3}} \times 10^{-8} \text{ cm.}$$

For triangular lattices the following values of $n$ occur: simple lattice, $n = \frac{1}{2}$; two interpenetrating lattices, $n = 1$; three interpenetrating lattices, $n = \frac{3}{4}$ or $\frac{2}{3}$; four interpenetrating lattices, $n = 2$.

Values of $a$, $a/2$, (the spacing of the $1\bar{\bar{2}}10$ planes), $a\sqrt{3}/2$ (the spacing of the $10\bar{1}0$ planes and $ca$ (the height of a cell) have been computed and are given in columns 2, 3, 4 and 5 of table 1. When $h$ is the distance of the plate from the axis of rotation, $x$ the distance of a given line from the undeviated central image, $d$ the distance between planes in the crystal, $\lambda$ the wave-length of the radiation and $N$ the order of the spectrum $x/h = N\lambda/d$, giving the relations $d = N\lambda h/x$ and $x = N\lambda h/d$. The wave-length used was the $K$ line of tungsten $\lambda = .211 \times 10^{-8}$ cm. In certain cases $h$ was 19.00 cm., in others 14.75 cm. Values of $x$ corresponding to these values have been calculated for the three fundamental spacings of each of the forms having values of $n$ already givin. They are shown in columns 6 to 11 of table 1. The values of $x$ determined from the four plates used in the calculations and the corresponding values of $a$ are given in tables 2 to 5. The average value of $a$ is $4.74 \times 10^{-8}$ cm. indicating four interpenetrating lattices. From talb 4 it appears that the 0001 spacing is $c/2$, i.e., the four sets of basal planes occur in pairs. A number of plausible models having such arrangement exist. They may be differentiated by the spacings of the pyramidal planes. It may be shown that in interpenetrating triangular lattices pyramids having indices of the form $(n\bar{O}\,{}^{-}p)$ have spacings according to the relation

TABLE 1

| 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 |
|---|---|---|---|---|---|---|---|---|---|---|
| $n$ | $\times 10^{-8}$ cm. | | | | h = 19.00 cm. | | | h = 14.75 cm. | | |
| | $a$ | a/2 | $a\frac{3}{2}$ | ca | $x_{11\bar{2}0}$ | $x_{10\bar{1}0}$ | $x_{0001}$ | $x_{11\bar{2}0}$ | $x_{10\bar{1}0}$ | $x_{0001}$ |
| $\frac{1}{2}$ | 3.01 | 1.50 | 2.60 | 4.22 | 2.67 | 1.54 | 0.95 | 2.07 | 1.20 | 0.74 |
| $\frac{3}{4}$ | 3.44 | 1.72 | 2.98 | 4.83 | 2.33 | 2.35 | 0.83 | 1.81 | 1.04 | 0.65 |
| 1 | 3.79 | 1.89 | 3.38 | 5.32 | 2.11 | 1.18 | 0.76 | 1.65 | 0.92 | 0.59 |
| $\frac{3}{2}$ | 4.33 | 2.16 | 3.75 | 6.08 | 1.85 | 1.07 | 0.66 | 1.44 | 0.83 | 0.51 |

$$d_{\text{nO}\bar{\text{n}}\text{p}} = K\frac{a}{2}\sqrt{3} \sin.\, \phi_{\text{nO}\bar{\text{n}}\text{p}},$$

where $\phi_{\mathrm{nO\bar{n}p}}$ is the angle between the planes and the basal pinacoid 0001 and $K$ is a factor varying from pyramid to pyramid in the same model and with different models for the same pyramid, its value depending upon the grouping of the planes in a given form. Values of this quantity have been determined by inspection of the models and are tabulated in table 6. The data of table 4 are consistent only with the distances calculated for model IV, which is therefore taken to represent the structure. In the tabulations but four models have been considered as these are all that satisfy reasonable considerations of symmetry.

TABLE 2

$h$ = *19.00 cm.*

| INDEX | $x$ | $d \times 10^8$ cm. | $a \times 10^8$ cm. |
|---|---|---|---|
| $10\bar{1}0$ | 0.97 | 4.13 | 4.77 |
| | 1.98 (2) | 4.05 | 4.68 |
| | 2.93 (3) | 4.11 | 4.74 |
| $11\bar{2}0$ | 1.70 | 2.36 | 4.72 |
| | 3.40 (2) | 2.36 | 4.72 |
| $01\bar{1}0$ | 0.95 | 4.22 | 4.87 |
| | 1.92 (2) | 4.18 | 4.82 |
| | 2.90 (3) | 4.15 | 4.79 |
| $\bar{2}110$ | 1.70 | 2.36 | 4.72 |
| | 3.40 (2) | 2.36 | 4.72 |
| $\bar{1}010$ | 1.95 (2) | 4.12 | 4.76 |
| Average.................. | | | 4.76±0.04 |

TABLE 3

$h$ = *14.75 cm.*

| INDEX | $x$ | $d \times 10^8$ cm. | $a \times 10^8$ cm. |
|---|---|---|---|
| $\bar{2}110$ | 1.34 | 2.32 | 4.64 |
| | 2.70 (2) | 2.31 | 4.62 |
| $10\bar{1}0$ | 2.20 (3) | 4.25 | 4.91 |
| | 3.00 (4) | 4.15 | 4.79 |
| $\bar{1}100$ | 1.50 (2) | 4.15 | 4.79 |
| $\bar{2}110$ | 1.35 | 2.31 | 4.62 |
| Average.................. | | | 4.73±0.10 |

TABLE 4

$h$ = *14.75 cm.*

| INDEX | $x$ | $d \times 10^8$ cm. | $a \times 10^8$ cm. |
|---|---|---|---|
| 0001 | 0.98 | 3.25 | 4.78 |
| | 1.92 (2) | 3.24 | 4.82 |
| $01\bar{1}0$ | 1.50 (2) | 4.15 | 4.79 |
| | 2.25 (3) | 4.15 | 4.79 |
| $10\bar{1}2$ | 1.22 | 2.55 | 4.68 |
| | 2.45 (2) | 2.54 | 4.67 |
| $30\bar{3}4$ | 2.90 | 1.07 | 4.81 |
| $11\bar{2}0$ | 2.65 (2) | 2.34 | 4.68 |
| $30\bar{3}2$ | 4.5–5.2 | 1.19–1.38 | |
| $20\bar{2}1$ | 4.80 (2) | 1.30 | 4.72 |
| $30\bar{3}1$ | 4.0–4.5 | 0.69–0.78 | |
| $40\bar{4}1$ | 5.5–6.0 | 1.04–1.13 | |
| Average.................. | | | 4.74±0.06 |

TABLE 5

$h$ = *14.75 cm.*

| INDEX | $x$ | $d \times 10^8$ cm. | $a \times 10^8$ cm. |
|---|---|---|---|
| 0001 | 1.85 (2) | 3.38 | 4.78 |
| | 2.80 (3) | 3.34 | 4.76 |
| $10\bar{1}1$ | 1.5–2.0 | 3.1–4.2 | |
| | 5.40 (6) | 3.46 | 4.70 |
| $10\bar{1}2$ | 2.40 (2) | 2.58 | 4.75 |
| $10\bar{1}3$ | 1.60 | 1.94 | 4.72 |
| $30\bar{3}2$ | 2.50 | 1.24 | 4.70 |
| Average.................. | | | 4.72±0.04 |

The investigation shows that ice is properly assigned to the hexagonal system, that it consists of four interpenetrating triangular lattices, and that the fundamental spacings are

$$a = 4.74 \times 10^{-8} \text{ cm.}; \ h = 6.65 \times 10^{-8} \text{ cm.}$$

$$d_{11\bar{2}0} = 3.79 \times 10^{-8} \text{ cm.}; \ d_{10\bar{1}0} = 2.37 \times 10^{-8} \text{ cm.}; \ d_{0001} = 3.32 \times 10^{-8} \text{ cm.}$$

The arrangement of the lattices is conveniently explained by referring the origin of each lattice to two unit axis making an angle of 120° and a third mutually perpendicular to these. The coordinates of the origins are then

$$0,0,0; \ \tfrac{1}{3},\tfrac{2}{3},1/z; \ \tfrac{1}{3},\tfrac{2}{3}, \ \tfrac{1}{2}; \ 0,0, \ (z+2)/2z.$$

TABLE 6

VALUES OF $K$ AND CORRESPONDING APPROXIMATE VALUES OF $x$ WHEN $h$ = 14.75 CM.

| INDEX | $\phi$ | Sin $\phi$ | MODEL I | | MODEL II | | MODEL III | | MODEL IV | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | $K$ | $x$ | $K$ | $x$ | $K$ | $x$ | $K$ | $x$ |
| $10\bar{1}1$ | 58°18′30″ | 0.8509 | $\frac{1}{2}$ | 1.8 | 1 | 0.9 | 1 | 0.9 | 1 | 0.9 |
| $10\bar{1}2$ | 39° 9′ 9″ | 0.6293 | 1 | 1.2 | 1 | 1.2 | 1 | 1.2 | 1 | 1.2 |
| $10\bar{1}3$ | 28°22′ 0″ | 0.4751 | $\frac{1}{2}$ | 3.1 | 1 | 1.6 | 1 | 1.6 | 1 | 1.6 |
| $20\bar{2}1$ | 72°50′20″ | 0.9555 | $\frac{1}{4}$ | 3.2 | $\frac{1}{2}$ | 1.6 | $\frac{1}{4}$ | 3.2 | $\frac{1}{3}$ | 2.4 |
| $30\bar{3}1$ | 78°22′13″ | 0.9795 | $\frac{1}{6}$ | 4.5 | $\frac{1}{3}$ | 2.2 | $\frac{1}{6}$ | 4.5 | $\frac{1}{6}$ | 4.5 |
| $30\bar{3}2$ | 67°37′36″ | 0.9248 | $\frac{1}{3}$ | 2.6 | $\frac{1}{3}$ | 2.6 | $\frac{1}{3}$ | 2.6 | $\frac{1}{3}$ | 2.6 |
| $30\bar{3}4$ | 50°32′15″ | 0.7720 | $\frac{1}{3}$ | 2.9 | $\frac{1}{3}$ | 2.9 | $\frac{1}{3}$ | 2.9 | $\frac{1}{3}$ | 2.9 |
| $40\bar{4}1$ | 81°13′30″ | 0.9886 | $\frac{1}{8}$ | 6.6 | $\frac{1}{3}$ | 2.2 | $\frac{4}{9}$ | 3.3 | $\frac{1}{4}$ | 3.0 |

The values of $z$, that is the relative displacement of the two planes making up a basal pair, is uncertain and needs further investigation. This requires a careful determination of the relative intensities of the spectra of different orders reflected from the 0001 planes and was beyond the scope of the present investigation. Conditions of symmetry suggest a value of $z = 6$, but this is purely conjectural.

The investigation was pursued under the direction of Prof. A. G. Webster of Clark University in the Physics Laboratory of the Worcester Polytechnic Institute.

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[1] *Paris, C.-R. Acad., Sci.*, **157**, Nov. 17, 1913, (924–926).

[2] Dana, *System of Mineralogy*, 1888 ed., p. 205.

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# *FRINGING REEFS OF THE PHILIPPINE ISLANDS.*

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Read before the Academy, April 23, 1918

A series of large-scale charts recently published by the United States Coast and Geodetic Survey for certain parts of the Philippine islands are, apart from their value to commerce, of much scientific interest in connection with the coral-reef problem by reason of the great volume of carefully ascertained facts that they present. They are in various respects more detailed and more accurate than the Admiralty and Hydrographic Charts hitherto available. Many of the islands are thus shown to have more or less minutely embayed shore lines, indicative of the subrecent or recent submergence of an eroded land surface. This is particularly true of Palawan, the southwesternmost member of the group, which has a shore line of most intricate pattern where its western side is indented by Malampaya sound: there can be no question that the coastal features of this kind exhibited on Palawan and many other members of the Philippine group result from the recent partial submergence of an uneven land surface.

According to Darwin's theory of coral reefs, in the form usually presented, shores of submergence should be fronted by barrier reefs; but the Malampaya district of Palawan is not so fronted; its reefs, where they occur, belong to the fringing class, and since Darwin's time fringing reefs have been associated with stationary or emerging shores. Barrier reefs are indeed exceptional in the Philippines, in spite of the repeated occurrence of embayed shore lines on many islands, and the question therefore arises whether the theory of upgrowing reefs on intermittently subsiding foundations is incorrect or incomplete. The object of this paper is to point out that the theory, as ordinarily stated, is incomplete, and that the element needed to complete it is to be found in a seldom-quoted passage from Darwin's own writings, as follows: "If during the prolonged subsidence of a shore, coral-reefs grew for the first time on it, or if an old barrier-reef were destroyed and submerged, and new reefs became attached to the land, these would necessarily at first belong to the fringing class" (Coral Reefs, 124).

This passage may be understood as meaning that the "prolonged subsidence" of an island might be too rapid to permit reef growth, until a pause allowed the establishment of a fringing reef; and also that the rapid subsidence of an island would destroy and submerge a barrier reef previously formed around it during slower subsidence, whereupon a fringing reef would be formed on the new shore line. Thus interpreted, the passage affords a satisfactory explanation of the frequent association of fringing reefs with shore lines of submergence in the Philippines and in certain other archipelagoes, even though such reefs may elsewhere be found on stationary shore lines or on shore lines of emergence. Fringing reefs thus formed may be described as "of a new generation:" they will evidently lie unconformably on the eroded rocks of the shore belt, and their unconformity, as well as the embayments of the shore line, will indicate their association with submergence. The contact of fringing reefs with the marine sediments that ordinarily characterize a non-embayed shore line of emergence would be essentially conformable.

Whether the shores of the Philippines now for the first time have reefs formed upon them, according to the first clause in the above quotation from Darwin, or whether the existing fringes are the successors of "destroyed and submerged barrier reefs," according to the second clause, may be determined for certain islands by the abundant off-shore soundings. Thus in the case of Palawan, its embayed western coast does not descend rapidly to great depths, but is fronted by a well defined submarine platform, 20 or 30 miles wide, along the seaward edge of which a discontinuous rim rises towards but not to the surface; the rim can be most reasonably explained as an incomplete upgrowth from a barrier reef of an earlier generation on the outer margin of a broad lagoon. It is not possible, in our present ignorance of the geology of Palawan, to determine whether the drowned barrier reef was formed by upgrowth during slow and long continued subsidence, or by outgrowth during a long-enduring still-stand of the island; but in either case, the great breadth of the lagoon plain appears to be the product of a long lasting process, and thus contrasts strongly with the new fringing reefs of the Malampaya district, which are so narrow as to be inconspicuous on charts of the largest scale. Evidently, therefore, the rapid submergence by which the barrier reef of Palawan was drowned must be of recent date.

Similar conclusions may be derived from other parts of the Philippines, where embayed shore lines, relatively narrow fringing reefs, and well defined submarine platforms are frequently found, although no islands are so striking in these respects as Palawan. The platforms cannot be reasonably ascribed to marine abrasion during a higher stand of the islands or a lower stand of the ocean, for the island shores are not clift; and the submergence of the platforms cannot be accounted for by a rise of ocean level, which must everywhere be of the same date, amount and rate, for the platforms vary in depth, and the new fringing reefs vary in breadth. The depth of the Palawan platform for example, increases from 25 or 30 fathoms at its southwestern end to 55 or 60 fathoms near its middle, and then decreases again toward the northeastern end; and the fringing reef, which is hardly chartable near the mid-length of the island where the platform is deep, has a width of 1 or 2 miles at the southwestern end of the island where the platform is relatively shallow.

On the other hand, the northeastern coast of Samar, on the opposite side of the archipelago from Palawan, has a moderately sinuous shore line with delta flats that diminish the initial size of its bays, and fringing reefs that reach forward a mile or so from its points; here the latest submergence cannot be so recent as that of Palawan. But instead of being benched by a submerged platform, the sea bottom off shore from Samar sinks rapidly to a great depth. Moreover, there are long stretches of the coast of Luzon which are neither embayed by arms of the sea, nor enclosed by barrier reefs, nor fronted by submarine platforms: Luzon, unlike many other of the Philippines, has a considerable extent of coastal lowlands, as if the growth of fringing reefs, or the outwash of detritus, or the emergence of the former sea border had increased its low littoral area. Again, Cebú and Negros, which occupy a somewhat central position in the archipelago, are described by Becker as terraced with elevated reefs up to altitudes of 2000 feet or more. The diverse shore features of the different islands are therefore best explained by local changes of land level, unlike in date, in direction, in amount and in rate.

It is particularly the rapid rate, the recent date, and the considerable amount of subsidence often indicated that are of significance in the coral-reef problem; and this is true not only for the Philippines but also for the other archipelagoes between Asia and Australia. Embayed shore lines indicative of submergence are common though by no means universal in all this region, but well developed barrier reefs are rare. Before the reefs of Cebú and Negros were elevated, and before the platform of Palawan was depressed, barrier reefs must have been more extensive than now in the Philippines, and possibly in the other island groups also; but today no examples of these forms are to be found in the archipelagoes that can compare with the great barrier reefs of northeastern Australia and of New Caledonia; few of the many small islands in the archipelagoes are enclosed by well developed encircling reefs, like those of the Fiji and Society groups; and atolls, which are so striking a feature of the open Pacific, are relatively uncommon in the archipelagoes.

The best explanation of the small development of barrier reefs and atolls in the archipelagoes is to be found, not in the lack of subsidence, which is elsewhere so intimately associated with reef development, for geological and physiographic evidences of subsidence abound on many islands; and surely not in the prevalence of unfavorable conditions as to the temperature and purity of sea water, for fringing reefs flourish; but largely in the occurrence of subsidence of so rapid a rate and in some cases of so great an amount, as to have submerged pre-existing reefs. Moreover the subsidences appear to be in many cases of so recent a date that the new fringing reefs are still narrow; it is presumably for this reason that the drowned barrier reefs and atolls have not yet had time to grow up again to sea level. Added to this is the frequent occurrence of recent uplifts in the archipelagoes, whereby weak marine sediments, overwashed by an abundance of alluvium from rejuvenated rivers, have come to occupy the shore line of certain islands, thus discouraging even the growth of fringing reefs, as around much of the coast of Borneo and along the southern coast of Java; but this aspect of the problem cannot be discussed here.

In view of these facts and inferences the Australasian archipelagoes must be considered much more unstable than the floor of the central Pacific. In that vast region, where reef upgrowth has generally kept pace with changes of level, and where atoll and barrier-reef lagoons have been filled with sediments to a moderate depth, Darwin appears to have been right in concluding that "the subsidence thus counterbalanced must have been slow in an extraordinary degree" (115). Not only movements of depression but modern movements of elevation also have been of moderate measure in the central Pacific, for none of the occasional elevated atolls found there have an altitude of more than a few hundred feet: it is only in Tonga and Fiji that greater measures of modern uplift are recorded, the greatest altitude of a reef in Fiji being 1030 feet: uplifted reefs in the Australasian archipelagoes are found at altitudes of 2000 and 3000 feet.

On the other hand, the instability of the Australasian region is attested not only by the evidence afforded by coral reefs, but also as above noted, by many geological researches; those by Molengraaff and Abendanon are particularly instructive in this respect, and fully confirm Darwin's statement:—"North of Australia lies the most broken land of the globe, and there the rising parts are surrounded and penetrated by areas of subsidence" (143). Hence while Darwin's general conclusion that "the rate of subsidence has not exceeded the upward growth of corals" (115) seems to hold true for the central Pacific, it is not valid for the region of the archipelagoes. The not infrequent occurrence there of subsidences at a rate sufficient to drown coral reefs ought to satisfy those objectors to Darwin's theory who have urged that it demands too great a uniformity, if not also too slow a rate in the movements of the earth's crust in oceanic areas. But let it be noted that rapid movements of subsidence, and also fringing reefs of a new generation which are formed along a shore where rapid subsidence had drowned pre-existing reefs, were explicitly recognized by Darwin as of possible occurrence—witness the first quotation from his Coral Reefs, above.

Fringing reefs of a new generation on shores of submergence should therefore be accepted as accounted for by Darwin's theory quite as well as other fringing reefs, and indeed as contributing their share towards verifying the theory, even though its author did not recognize any actual fringing reefs as of this origin. He colored all fringing reefs red on his chart, as indicating non-subsiding coasts; for even after pointing out the possibility of their formation where "prolonged subsidence" has taken place, he added: "I have no reason to believe that . . . . any coast has been coloured wrongly with respect to movement indicated" (124). His information about the Philippines was scanty; he colored all their fringing reefs red, as indicative of stationary or rising coasts.

There can indeed be little doubt that a number of other islands also, which have unconformable fringing reefs along their embayed shores, were wrongly classed by Darwin, who took no account of embayments or of unconformities. Striking instances of this kind might be pointed out in the Solomon group, where the small island of Fauro, for example, which is described by Guppy as a deeply denuded volcanic wreck, has a shore line of marked irregularity, a narrow fringing reef, and a well developed submarine platform from 40 to 70 fathoms in depth: such a combination of features proclaims intermittent subsidence after long-continued subaerial erosion, the last movement being more rapid than reef upgrowth; yet all the members of the Solomon group are colored red on Darwin's chart, because the little information he had about them gave "a presumption that they are fringed" (167). Other examples of new fringing reefs on shores of submergence are found on the granitic islands of the Seychelles in the western Indian ocean, as will be again noted below. The

Andaman islands in the Bay of Bengal are elaborately embayed, and a submarine platform several miles in breadth and from 20 to 40 fathoms in depth adjoins them; a bank of similar depth, measuring 35 by 10 miles lies not far away to the east; yet these islands have no barrier reefs and only very narrow and discontinuous fringing reefs; their submergence must be very recent. If the postglacial rise of ocean level were the cause of all these submergences, reefs should everywhere be of about the same volume: as a matter of fact they vary in volume enormously. The fringing reefs of southwest Palawan, of Yap in the western Pacific, and of Rodriquez in the southern Indian ocean are two or three miles wide; various atoll and barrier reefs are half a mile or a mile wide. Others are much narrower, and still others are discontinuous, or so imerfectly developed as not to reach sea level: finally, some submarine banks are flat, without any reef rim. These irregular values speak for variable subsidence of the reef foundations, not for a uniform rise of ocean level.

The occurrence of subsidence in the Australasian region at a more rapid rate than that of coral upgrowth has an interesting bearing on the origin of the numerous submarine banks, of various depths down to 40, 50, or 60 fathoms, in the China sea. It is evident that if islands suffer a movement of subsidence rapid enough to drown their barrier reefs and thus to develop fringing reefs of a new generation, the same subsidence would completely submerge neighboring atolls. Furthermore, if the rapid subsidence of a group of islands were of so recent a date that the resulting fringing reefs are still narrow, the drowned reefs of the submerged atolls might still remain below sea level, even if the amount of submergence had not been great enough to kill all their corals.

Now in view of the proximity of the China sea to the Philippine Islands, it seems reasonable to suppose that its floor has shared some of the many movements by which the islands of the archipelago have been disturbed; hence the submarine banks of that deep basin are best explained, following Darwin's theory, as drowned atolls not yet rebuilt. The date as well as the rate and the amount of a subsidence is therefore of importance in determining whether the atolls that it drowns shall still be submerged. Certainly recent submergence, presumably due to subsidence, has affected the Macclesfield and certain other large submarine banks of the China sea, for corals are growing on the rims of many of them, but have not yet built the rims up to the sea surface. On the other hand in spite of the proximity of the unstable Philippines, the Glacial-control theory explains the Macclesfield bank, which is taken to be a typical example of its kind, as the remains of a huge volcanic cone, originally as large as Hawaii, which stood still long enough to be worn down to small relief in preglacial time, and which, still fixed, was truncated by abrasion at a lower level while the ocean was depressed about 40 fathoms during the Glacial period. So long enduring a stability seems improbable enough even for the central Pacific, and much more improbable for a next-door neighbor of the Philippines.

It may be noted that if the surface forms of the mid-Pacific atolls are considered alone, they can be accounted for very satisfactorily by the Glacial-control theory, which was invented chiefly to explain the special features that they present: but as atolls occur in association with barrier reefs in the Caroline, Society, Fiji and other groups, and as the central islands within the barrier reefs present features which, although perfectly accounted for by Darwin's theory of intermittent subsistence, cannot be accounted for by the Glacial-control theory, its apparent success in explaining atolls is thereby discredited, all the more so in view of the recent discussion by Skeats of the boring in the Funafuti atoll (*Amer. J. Sci., New Haven,* **45,** 1918, 81–90).

The chief value of the ingenious Glacial-control theory may therefore be found not so much in its postulate of the prevalent stability of reef-bearing islands, or in its assumption that reef corals were killed and that reef-bearing islands were abraded while the ocean was chilled and lowered in the Glacial period, but in the emphasis that it gives to changes of sea level from climatic causes as a factor in the coral-reef problem; for it is manifest that if the post-glacial rise of sea level coincide in time with the subsidence of an island, the resulting submergence will be at an accelerated rate and of an increased amount; while if the fall of sea level occasioned by the oncoming of a glacial epoch coincide with a subsidence, the resulting submergence will be at a retarded rate and of a decreased amount. It cannot however be supposed that two processes so unlike in cause as external climatic changes and internal crustal deformations should be closely related in time; their coincidences must be fortuitous. Throughout the central Pacific the rate and amount of recent submergence have not been as a rule too great to be compensated by reef upgrowth; witness the abundant atolls and barrier reefs. But in the region of the Australasian archipelagoes compensation of submergence by reef upgrowth has frequently been unsuccessful; witness the rarity of well developed barrier reefs and the almost entire absence of atolls. As the climatic changes of ocean level must have been everywhere the same, the factors which have determined the success or the failure of reef upgrowth would appear to be the rate, the amount and the date of subsidence.

It may be added that submarine banks, of such form that they are best accounted for as drowned atolls, are rare in the Pacific. A group of ten or more of them is known in an island-free space north of Fiji: several extensive banks also occur in Tonga. Exception must therefore be made in favor of a rapid submergance only for these relatively few examples of submerged Pacific atolls, and the rule that the great majority of Pacific atolls have not been submerged faster than the rate of reef upgrowth is thereby proved. In the Indian ocean, on the other hand, the number of submarine banks bears a larger proportion to that of atolls, and the Indian ocean is generally regarded by geologists as the seat of greater and more recent movements of depression than the Pacific. Recent and rapid subsidences of moderate amounts may therefore be plausibly regarded as of more general occurrence in the Indian than in the Pacific ocean. The recent date and the rapid rate of subsidence appear to be of greater importance than its amount in the case of the Great Chagos bank, where the submergence does not seem great enough to drown the reef-building corals. Here the muddy central area is 40 or 50 fathoms deep; it is bordered by an irregular sandy bank from one to 5 miles or more in breadth and from 15 to 20 fathoms in depth, on the outer margin of which rises a rim about a mile in width, and only 5 or 10 fathoms in depth; singularly enough, there is little living coral on the outer rim, though knobs of growing coral rise from the central depression. The diameters of the whole mass range from 50 to 75 miles: its form suggests that a prolonged stationary period, during which a broad atoll-reef was developed, was followed by a subsidence of about 10 fathoms, after which a shorter stationary period permitted the upgrowth of a narrower reef; then a rapid and presumably recent subsidence of 5 or more fathoms ensued, since which no effective reef growth has taken place, possibly because, according to Daly's suggestion, the submerged corals were smothered by wave- and current-shifted sediments.

Unfortunately no archipelagoes comparable to those of the Australasian region are present in the Indian ocean to give evidence in the case, but it may be noted that a few high islands which occur in association with the Indian ocean banks—chiefly the granitic islands in the area of the great Seychelles bank—have narrow and unconformable fringing reefs on their deeply eroded and well embayed shores; thus they repeat in a small way the more abundant and therefore more compulsory evidence that is provided by the charts of the Philippines. Further details on these topics are given in an article on "Submarine Banks and the Coral Reef Problem," now in course of publication in the *Journal of Geology*, and in an article on the "Subsidence of Reef-encircled Islands," soon to appear in the *Bulletin of the Geological Society of America.*

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# *DILATION OF THE GREAT ARTERIES DISTAL TO PARTIALLY OCCLUDING BANDS*

By William S. Halsted

Medical School, Johns Hopkins University

Read before the Academy, April 22, 1918

The incentive to the work was primarily the desire to cure aneurysms of the abdominal aorta and common iliac arteries.

The method usually employed for the cure of aneurysm is the simplest, viz., the ligation of the affected artery proximal and as close as feasible to the aneurysm. The aorta has been ligated 25 or more times in man, and always with fatal result. Death has been due to hemorrhage or overtaxed heart. Neither gangrene nor paraphlegia has ever resulted from ligation of the aorta in man. We found, in dogs, as was to have been expected, that fine, completely occluding, ligatures (sizes C or E sewing silk) applied to the thoracic aorta just below the arch would cut through in about two days, and invariably with promptly fatal hemorrhage; whereas coarse ligatures usually made their way through the aorta wall very slowly and without leakage of blood. A connective tissue diaphragm often forms in the wake of these broader threads and the lumen of the vessel may be more or less completely reëstablished.

It occurred to me after much experimentation that occlusion of the aorta to a degree not sufficient fatally to overburden the human heart might effect the cure of an aortic aneurysm. Knotted ligatures we found to be unsuitable, for a desired degree of constriction or obliteration could not be accurately obtained nor could the crushing of the arterial wall be invariably avoided. Tapes of various materials were tested—of cotton, of chromicized intestinal submucosa, of elastic tissue obtained from the aorta, of aponeurotic white fibrous tissue. These were applied in spiral or cuff form. Best suited to the purpose were bands of metal, of aluminum, accurately rolled in cylindrical form by a little instrument of this kind (exhibit). In the use of these metal bands it was impossible to crush the arterial wall, and the desired amount of obturation could be obtained with precision, and also maintained.

The infolded and snugly opposed intimal surfaces under the compressing band have in no instance adhered to each other, and for the reason that the pressure necessary to produce even a very slight reduction in the lumen of the vessel has, in my experience, invariably caused atrophy of its wall. When the occlusion is complete the necrotic arterial wall included in the metal band becomes replaced by a solid cylindrical cord of fibrous tissue, the substitution taking place from the ends.

An interesting incidental observation which we have made in the course of our experiments with the metal band is this; that a dilation of the artery occurs just below a band when the degree of constriction is of the proper amount. This observation apparently explains in a measure the occurrence of aneurysms of the subclavian artery distal to a cervical rib. Analyzing 525 clinical cases of cervical rib we found 106 in which the subclavian artery had been compressed, and that in 21 of these, aneurysm or dilation of this vessel distal to the site of constriction had been noted.

As to the cause of these aneurysms, five of which have come to the knowledge of the collators, there has been varied conjecture. Commentators are, however, agreed that the occurrence of the dilation would have been less incomprehensible to them had it manifested itself on the proximal instead of the distal side of the compression. Attempts have been made to explain the phenomenon, and the following suggestions offered as to its possible cause:

(1) Weakening of the wall of the subclavian artery from erosion by the rib.

(2) Variable or intermittent pulse pressure occasioned by the normal excursions of the arm.

(3) Vasomotor and vasa vasorum disturbances leading to modified nutritional activities in the wall of the artery.

In casting about for an explanation of these aneurysms there constantly obtruded itself the picture of the dilated arterial trunks which, I find from the study of about 400 cases, has occasionally been noted on the cardiac side of arterio-venous fistulae. In our own clinical and experimental cases, dilation of the artery proximal to the fistula has occurred invariably. For this remarkable manifestation, likewise, no satisfactory cause has been assigned. There might, I thought, be a common cause for both—for the dilation of the subclavian artery distal to the cervical rib, and for the dilation, central to the arterio-venous fistula, of the artery concerned in its formation. Hence, for a number of years, in the course of various experiments in partial occlusion of the arteries, I had somewhat in view the possibility of the production, beyond the point of constriction, of a dilation of the artery, analogous to the dilations which have been observed in cases of cervical rib.

Four years ago when after many trials I had altogether despaired of having the hope realized, I was startled, on examining the abdomen of a dog whose aorta had been constricted for about six months to see that each of the branches of trifurcation had become dilated almost to the size of the main aortic trunk.

With this observation as incentive, Dr. Mont Reid and I, the following winter, constricted the abdominal aorta just above its trifurcation, in many dogs and at intervals explored and reexplored the abdominal cavities, but with negative result. Finally, on investigating the abdomen of the last dog we found the hoped-for dilation. The degree of obturation of the aorta was accurately determined on sacrificing the animal, and the following year the experiments were more advantageously repeated because of the data obtained from this case. Now, that we have apparently determined the relative amount of constriction required to give the most pronounced results we are able in almost every instance to produce the dilation.

As regards the cause of the dilation produced experimentally we may I think, conclude that it is not to be found in any of the three factors which have been proposed as responsible for the dilation observed in cases of cervical rib, viz., (1) vasomotor paralysis, (2) trauma and (3) variable blood pressure.

Ad. 1. *Vasomotor Paralysis.* (a) The vasomotor nerves and the vasa vasorum are destroyed by the moderately constricting and totally occluding bands quite as surely as by those which, occluding almost totally, have produced the greatest amount of dilation. (b) Only a portion of the circumference of the subclavian artery is exposed to the pressure of the cervical rib and the scalenus anticus muscle and hence only a fraction of the vasomotor nerves or vasa vasorum could be pressed upon.

Ad. 2. *Trauma.* (a) The dilation is usually fusiform and distal to the rib. (b) Trauma is excluded as a factor in the experimental dilations.

Ad. 3. *Variable Blood Pressure.* (a) Patients suffering from the pressurepain of cervical rib rarely make wide excursion movements of the arm. (b) The degree of occlusion is constant in the experimentally constricted vessel.

When an arterial trunk is ligated it becomes occluded to the first proximal and first distal branches and ultimately reduced to a fibrous strand.

From observations which we have made on man and dogs I am quite sure that there may be a remarkable fall in blood pressure in what I have termed 'the dead arterial pocket,' while there is still little if any sign of diminution in the caliber of this portion of the vessel. For example, the right common carotid was ligated by the writer in a case of aneurysm of the external carotid. About 3 months later, in the course of an operation for the excision of the uncured aneurysm, the internal carotid, dead-pocketed between the circle of Willis and the carotid ventricle, was freely exposed for a considerable distance. It had lost its cylindrical form, being flat and tape-like, and, although evidently possessing a considerable lumen, seemed to be empty. When incised, a few drops of blood oozed without pulse from the little cut. The artery was then resected. Its wall was thickened on one side but the lumen was still perhaps three times that of a radial artery. Similar observations I have made twice on the external iliac of the dog after occlusion of this vessel at its origin from the aorta. In the dead pocket between the aorta and the origin of the circumflex iliac and common trunk of the epigastric and obturator arteries the blood pressure must have been almost nil, because from a little slit in the apparently normal arterial wall of the relatively empty external iliac artery the blood escaped very slowly in a tiny, almost pulseless jet about 1 cm. high; whereas, from the femoral artery, below the profunda, the blood spurted normally from a similar knife-prick.

Hence in an artery doomed to obliteration, it would seem that the blood pressure may be lowered before the occlusion process sets in—the lowered pressure being, perhaps, the immediate factor leading to the obliteration.

Can these observations have any bearing upon the explanation of the dilation of the aorta above its trifurcation and of its triad branches in the dog after partial occlusion; of the dilation of the carotid in the human subject which I have observed in one case after partial occlusion of the innominate combined with ligature of the first and third portions of the right subclavian; and of the aneurysm of the third portion of the subclavian in cases of cervical rib?

In 1906 Dr. Richardson and I made the observation that after partial occlusion of the thoracic aorta the maximum pressure may be permanently lowered and the minimum pressure permanently increased distal to the constricting band; and in recent experiments Dr. Reid and I have observed that after constriction of the lower abdominal aorta the diastolic pressure may be so increased and the systolic pressure so lowered as to reduce the pulse pressure by nearly one half. The blood stream in this case, passing with greater velocity and less pressure through the band prevents the obliteration of the artery to the nearest branch, the pocket being not a dead one as it is in the case of total obliteration. The blood in this pocket beyond the constriction streams, presumably, in whirlpools, somewhat as in the vein and, also, as in the artery in arterio-venous fistula; the thrill, not palpable at first if the occlusion has been nearly complete, later may be perceived with the finger; and the bruit, always audible with the stethoscope, becomes louder as the peripheral arterial resistance increases.

To these factors, then—to the abnormal play of the blood in the relatively, as distinguished from the absolutely dead pocket and to the absence of normal pulse pressure, essential probably to the maintenance of the integrity of the arterial wall, we may have to look for the solution of our problem.

We have completely occluded the aorta just above the trifurcation only in dogs. Usually there has been no distal dilation, and in a previous paper I made the statement that dilation had not been observed below a totally occluding band. Since then, however, a slight degree of dilation, distal to the completely obturated vessel, has taken place in three instances. A dilation of this ventricle-like portion of the aorta between the band and the trifurcation might be expected even in case of complete occlusion, for the anastomosis is very free in this situation and the dead pocket is usually, and perhaps always too short to become obliterated. Lumbar branches may be given off just below, as they are just above the band.

In two instances I have made the following observation in testing, during the life of the animal, for the patency of the aorta under the band. Pressure with the finger immediately above the band shut off the pulse in what we term the ventricle; whereas, pressure with the back of the scalpel-blade, made as close to the band as possible, did not. In these cases there was a patent lumbar artery so close to the proximal edge of the band that pressure by the finger obliterated it, whereas, the knife blade which could be brought to bear on the aortic wall between this little artery and the upper edge of the band did not interrupt the flow in this important asastomotic branch. The contribution of this little artery to the anastomotic bloodstream was sufficient to convert an impalpable into a palpable pulse. A palpable pulse in the ventricle below the band is so invariable, whether the aorta has been completely occluded or not, that the patency of the artery under the band cannot be definitely determined during the life of the animal unless temporary occlusion of it between the band and the nearest lumbar artery obliterates or decidedly influences the pulse in the ventricle. If pressure above the band does not affect the pulse just below it we may conclude that obturation is complete.

Fortunately it occurred to me a few days ago to restudy, with reference to the possibility of finding depicted a dilation of an artery below a ligature, the sketches of surgeons who in bygone years had experimentally ligated the blood-vessels of animals. I was delightfully surprised to find, in the beautifully illustrated volume of Luigi Porta[1] published in 1845 two drawings which portrayed a pronounced dilation of the aorta and its ventricle immediately below the site of ligation. The ligatures in the two dogs had been applied eight and fifteen months before the death of the animals. There is a great bundle of dilated vessels—the vasa vasis—bridging the gap between the retracted ends of the divided aorta.

Thus three-quarters of a century ago this great, perhaps the greatest surgeon of Italy, furnished irrefutable proof of a remarkable phenomenon which must eventually have interest for the physiologist, the pathologist and the surgeon. Luigi Porta describes the drawing but makes no further comment upon the dilation.

Before the introduction of antiseptic surgery by Lister, thrombosis quite invariably followed ligation of an artery, and it was to the organization of the thrombus that the surgeon looked for the prevention of secondary hemorrhage and for the preservation of the life of the patient. If thrombi formed in these two cases of Porta they must have been eventually absorbed, for the distribution of the dilated vasa vasis proves that the aortic free ends were patulous, and we have further proof of this in the dilation of the aortic ventricle just below the site of the ligation.

In the course of my experiments in partial occlusion of the arteries I have often studied the illustrations, carefully I thought, in Luigi Porta's work, but not until I scanned them with the particular object in view did I discover the dilations so strikingly manifest. I wonder if anyone has ever commented upon or been interested in these two observations of Porta.

In the human subject I have in one instance observed a remarkable dilation of an artery distal to a partially occluding band. In this case an aluminum band was applied to the innominate artery for the cure of a subclavian aneurysm. A few weeks later, the aneurysm being uninfluenced by this procedure, the subclavian artery was ligated both proximal and distal to the sac, and a cure effected. Three years later a quite cylindrical dilation of the right common carotid was observed; and now, twelve years after the application of the band, the common carotid artery is strikingly dilated throughout its entire length. The band on the innominate can be palpated; the blood is coursing through it, and distal to the band is a distinct bruit (exhibit).

*Summary.*—1. A partially occluded artery (abdominal aorta, innominate, carotid, subclavian) may dilate distal to the site of constriction.

2. The dilation is circumscribed and has been greatest when the lumen of the artery (the aorta) was reduced to one-third or perhaps one-fourth of its original size.

3. When the obturation has been slight in amount dilation has not been observed; of 7 cases of complete obstruction there was a very moderate degree of dilation in 3, and none in 4.

4. Complete or partial occlusion of the thoracic aorta may be followed by dilation central to the point of constriction.

5. Dilation or aneurysm of the subclavian artery has been observed twenty-seven or more times in cases of cervical rib.

6. The dilation of the subclavian is circumscribed, is distal to the point of constriction, and strikingly resembles the dilation which we have produced experimentally.

7. The dilation of the artery proximal to an arterio-venous fistula and distal to a partially occluding band may prove to be referable to the same cause.

8. When the lumen of the aorta is considerably constricted the systolic pressure may be permanently so lowered and the diastolic pressure so increased that the pulse pressure may be diminished by one-half.

9. The experimentally produced dilations and the aneurysms of the subclavian artery in cases of cervical rib are probably not due to vasomotor paralysis, trauma, or sudden variations in blood pressure.

10. The abnormal, whirlpool-like play of the blood in the relatively dead pocket just below the site of the constriction, and the lowered pulse pressure may be the chief factors concerned in the production of the dilation.

11. Bands, rolled ever so tightly, do not rupture the intima.

12. Intimal surfaces, brought, however gently, in contact by bands or ligatures do not, in our experience, unite by first intention, for the force necessary to occlude the artery is sufficient to cause necrosis of the arterial wall.

13. The death of the arterial wall having been brought about by the pressure of the band, a gradual substitution of the necrotic tissue takes place, the new vessels penetrating it from both ends. It is, I believe, in this manner that an artery becomes occluded, and it is thus that a fibrous cord forms within the constricting band.

---

[1] Luigi Porta. Delle alterazioni patologiche delle arterie per la legatura e la torsione. Milano, 1845, pp. 350, 351, plate V, figs. 3 and 5.

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# ON THE CORRECTION OF OPTICAL SURFACES

By A. A. Michelson

Ryerson Physical Laboratory, University of Chicago

Read before the Academy, April 23, 1918

In a recent number of the *Philosophical Magazine*, an interesting method for correcting optical surfaces by means of the interferometer, was developed by Mr. Twyman. While nothing in the paper indicates that the method is limited to relatively small surfaces, it would appear that such an application to mirrors and lenses of the size of modern astronomical telescopes can hardly be contemplated as this would involve interferometers of at least equal dimensions.

It was hoped that a modification of Mr. Twyman's method, with an interferometer of usual size, could nevertheless be employed for large lenses or mirrors.

It was found, however, unless the two optical paths of the interferometers were equal, which would involve the presence of a second lens equal to the one to be corrected, that the circular interference bands are extremely small and difficult to observe.

The following simple, fairly direct method, obviates all these difficulties and has given excellent results.

A silt in the focus of the mirror or lens to be tested is illuminated by light from a Nernst glower, concentrated by means of a microscope objective and a total reflection prism. The light returns immediately above the prism forming an image of the slit which is viewed through a microscope with a $\frac{1}{12}$ inch objective.

A series of screens (an adjustable double slit would be much better) with two rectangular apertures are placed in succession in front of the lens or mirror to be tested; one of the apertures being central and the other at varying distances from the center.

The resulting diffraction figure will be a series of bands parallel with the slit, and the position of the central band (achromatic in white light) will remain constant if the adjustment is right and the mirror perfect.

The error in light waves will be half the observed error in fractions of the fringe-width.

The lens or mirror is rotated through the entire circumference, at intervals of 45 degrees or less, and the same operation repeated; and the results plotted on the corresponding chart, which gives accordingly the error in light-waves at every selected point of the surface.

This process applied to a 5-inch achromatic lens showed errors so small that artificial errors were introduced by placing in the path of the pencil a plane parallel plate which had been made roughly cylindrical by retouching locally.

The errors were then measured as described, and amounted to about seven-tenths of a light wave at the greatest. The corrector plate was again retouched by local polishing, and after a half dozen trials (time occupied being about six hours) the errors were reduced to the order of one- or two-hundredths of a light-wave; and the resulting image (which was badly astigmatic) was rendered practically perfect.

It is clear that such a process may be applied to even the largest astronomical mirrors or lenses, both in the original figuring and in the final correction; further, this final correction may be made upon the auxiliary plate, thus incurring no danger to the objective.

With evident modification the method applies to the correction of prisms and gratings. In the latter, however, since the light must be nearly homogeneous, there may not be sufficient intensity to observe the interference fringes under the high magnification required.

It may therefore be of advantage to apply the interferometer (replacing one of the mirrors by the grating) or even more simply, by observing the interference of the light reflected from a plane surface with that diffracted from the grating.

In either case, when the adjustment is perfect, the fringes are concentric circles—which remain constant when the eye or the observing telescope is moved about in any direction, if the grating is perfect; and if not, measurement of the diameters of the circles gives the error.

If, however, the difference of path in the interferometer is small, a curious singularity is presented. The interference fringes are no longer circles but complicated forms expressible by the formula:

$$\Delta = (y - y_0)\ (x^2 + y^2) + \alpha x + \beta y.$$

Further details will appear in a coming number of the *Astrophysical Journal.*

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# HEREDITARY TENDENCY TO FORM NERVE TUMORS

By C. B. Davenport

Station for Experimental Evolution, Carnegie Institution of Washington

Read before the Academy, April 23, 1918

The term multiple neurofibromatosis is applied to a rather rare condition characterized by the appearance of numerous sessile or pedunculated swellings or tumors of varying consistency and size. These may be present from birth, tend to grow larger and may even become confluent over a considerable area. Examination shows that they are fibrous tumors and frequently contain one or more nerve fibers or, if more deep seated, may be enlargements of the perineurium of nerve trunks. They are due to cell proliferations of the connective tissue sheaths of nerves.

The course of the disease is influenced by metabolic changes in the body. Thus pregnancy frequently stimulates growth of the small tumors which have been present from birth. In other cases even the changes of puberty are associated with the first marked development of the tumors. After various zymotic diseases, arsenical and lead poisoning, rapid growth of the tumors has been observed. The numbers of tumors may be very great, as many as 2000 or 4000. The tumors may be stimulated to grow by external mechanical agencies also, such as the irritation of a sword belt.

Associated with the tendency to form tumors is the production of pigmented spots or patches in the skin—of a café au lait color. Such patches when of small size may grow into colored moles or small tumors. On autopsy tumors are frequently found on the deeper lying nerves.

Multiple neurofibromatosis is a rare condition; it is found in only about 1 in 2000 cases that present themselves to medical clinics or private practitioners for skin diseases. Despite this there are many cases in the literature where 2 to 6 members of a family show some of the symptoms. The fact that only *blood* relatives are affected indicates that the disease is not a communicable one and it is equally certain that it is not induced by infection through the placenta. The two sexes are nearly equally apt to be affected—of 243 affected persons 56.8% are males. The disease tends to recur without a break in the generations and is equally apt to come down the male and the female line. Consequently it looks as though the hereditary factor in neurofibromatosis is a dominant one. In each affected fraternity, indeed, about 50% of the individuals are affected, as is expected if it is a dominant trait. Actually 43.5% were found affected. In some cases, however, a generation is skipped—a result that can be explained on the hypothesis of occasional failure of dominance.

The symptoms of neurofibromatosis are very diverse. But inside of one family they are apt to be alike. This speaks strongly for the hypothesis of an inheritance factor. Similarly the location of the principal tumors is apt to be the same in one family, although it shows the greatest diversity in different families. Other multiple tumors are inherited in the same way as neurofibromata. Thus the tendency to form vascular tumors of the skin and mucous membranes has been shown by Osler (1901) and others since to be a dominant one. Polyadenomata are inherited similarly. Likewise the tendency to form pigmented patches in the skin (ephilides) was shown by Hammer to be a dominant trait. To this same group of heredity belong epidermolysis bullosa, angioneurotic oedema, and persistent hereditary oedema, also such skin diseases as psoriasis, porokeratosis and ichthyosis.

In not a few cases the removal of neurofibromata has been followed by malignant growths, at the same spot. It is plain that neurofibromata are in some way related to cancerous growths. The fact that neurofibromata have an inheritable basis strengthens the view that cancers in general have such a basis.

The complete paper will be published jointly with Dr. S. A. Preiser.

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# *ARITHMETICAL THEORY OF CERTAIN HURWITZIAN CONTINUED FRACTIONS*

By D. N. Lehmer

Department of Mathematics, University of California

Communicated by E. H. Moore, April 2, 1918

The following research is the outcome of the discovery, made some three years ago, that the denominators of the convergents of order $3n$, $3n-2$, and $3n-6$, as well as the numerator of the convergent of order $3n-3$ in the regular continued fraction which represents the base of Naperian logarithms, are all divisible by $n$. The convergents recur modulo $n$ with a period of $6n$ when $n$ is odd, and with a period of $3n$ when $n$ is even.

To account for these curious theorems, and to place them in their proper setting, it was found necessary to study a more general type of continued fraction first investigated by Hurwitz.[1] The regular continued fraction for the Naperian base was discovered by Roger Cotes[2] to be (2, 1, 2, 1, 1, 4, 1, 1, 6, 1, 1, 8, 1, 1,....), or as we may write it $(2, 1, 2n, 1, 1)$, $n = 1, 2, 3, \ldots$ The proof of the remarkable sequence of partial quotients was first given by Euler,[3] by means of the theory of the Riccati equation. Euler established a number of other interesting expansions, such as:

$$\sqrt{e} = (\overline{1, 1, 4n+1}),\ n = 0, 1, 2, 3, \ldots.$$
$$\sqrt[s]{e} = (\overline{1, s\,(2n+1) - 1, 1}),\ n = 0, 1, 2, 3, \ldots.$$
$$\frac{\sqrt[s]{e}+1}{\sqrt[s]{e}-1} = (\overline{(4n+2)\,s}),\ n = 0, 1, 2, 3, \ldots.$$

The continued fractions studied by Hurwitz may be written in the form:

$$(q_1, q_2, q_3, \ldots q_r, \overline{\varphi_1(n), \varphi_2(n), \varphi_3(n), \ldots \varphi_k(n)}),\ n = 0, 1, 2, 3, \ldots.$$

where the 'irregular' partial quotients, $q_1, q_2, \ldots q_r$ are rational, and with the possible exception of $q_1$, all positive. The functions $\varphi_i(n)$ are rational integral functions whose degrees may, some or all, be zero. If the functions are all of degree zero, the fraction becomes an ordinary periodic continued fraction. The fractions discovered by Euler are seen to be Hurwitzian fractions where all the functions are of degree zero except one which is of the first degree. The general type of such a fraction with no irregular partial quotients is

$$(\overline{a_1, a_2, a_3, \ldots a_{k-1}, ma_k + b}),\ m = 0, 1, 2, \ldots.$$

and for such fractions I have been able to prove the congruences:

$$A_{2nk-1} - 1 \equiv B_{2nk} \equiv 0 \pmod{n},$$
$$A_{2nk} \equiv B_{2nk-1} \equiv (-1)^{nk-1} \pmod{n};$$

Where $A_m/B_m$ is the $m$th convergent to the continued fraction, and $n$ is any number prime to $A_k$, $A_{k-1}$ and $B_{k-1}$; $a_1, a_2, a_3, \ldots a_{k-1}$ being positive or negative integers or zero; $A_k$, $A_{k-1}$, $B_{k-1}$ positive or negative integers not zero.

The discussion is based on the following two theorems:

Theorem I. If $A_m/B_m$ is the $m$th convergent of $(\overline{a_1, a_2, \ldots a_{k-1}, ma_k})$, $m = 1, 2, 3, \ldots$ and $A'_m/B'_m$ is the $m$th convergent of $(\overline{a_{k-1}, a_{k-2}, \ldots a_2, a_1, -a_km - 2M})$, $m = 1, 2, 3, \ldots$; where $M = (B_{k-1} + A_{k-2})/A_{k-1}$; where $a_1, a_2, a_3 \ldots a_{k-1}$, are positive or negative integers or zero, and where $a_k$, $A_{k-1}$ are positive or negative integers, not zero, then:

$$A_{\rho k-1} = (-1)^{\rho-1} A'_{\rho k-1},$$
$$A_{\rho k} = (-1)^{\rho} (A'_{\rho k} + MA'_{\rho k-1})$$

for $\rho = 1, 2, 3, \ldots.$

Theorem II. With $A_m/B_m$ defined as in Theorem I, and $A''_m/B''_m$ defined as the $(m+1)$st convergent of

$$(K, a_{k-1}\, a_{k-2}, \ldots . a_2, a_1, L, \overline{a_{k-1}, a_{k-2}, \ldots . a_2, a_1, -a_k m - aM}),$$
$$m = 2, 3, 4, 5, \ldots ,$$

where $K = (B_{k-1} - A_{k-2}) / A_{k-1}$
and $L = -a_k - 2\,(B^2_{k-1} + B_{k-2}\, A_{k-1}) / A_{k-1}\, B_{k-1};$

$a_1, a_2 \ldots . a_k, A_{k-1}$ and $M$ as before, and $B_{k-1}$ not zero, then:

$$B_{\rho k-1} = (-1)^{\rho-1} A''_{\rho k-1},$$
$$B_{\rho k} = (-1)^{\rho} (A''_{\rho k} + MA''_{\rho k-1})$$

for $\rho = 1, 2, 3, \ldots.$

These two theorems are proved by complete induction. The proof of each proceeds in an unusual and interesting way by assuming both equations of the theorem to hold for $\rho \leqq n$, then proving that the first of the two equations must be valid for $\rho = n + 1$, and then using this result to show that the second holds also for $\rho = n + 1$.

The three fractions considered in these two theorems are then discussed with respect to a modulus $n$ prime to $2a_k$, $A_{k-1}$ and $B_{k-1}$. It is possible to find an even value of $m$, say $m = 2\lambda$, which shall be less than $2n$, and which shall satisfy the congruence

$$a_k m + 2M \equiv 0 \pmod{n}$$

This value of $m$ will provide a partial quotient in the second fraction of Theorem I, which shall be congruent to zero modulo $n$. The two continued fractions of this theorem are then seen to be inverse modulo $n$ as far as this partial quotient. From the properties of inverse continued fractions, we have then $A_{2k\lambda-1} \equiv A'_{2k\lambda-1}$. But by Theorem I itself $A_{2k\lambda-1} = -A'_{2k\lambda-1}$ so that we derive $A_{2k\lambda-1} \equiv 0 \pmod{n}$.

It is possible, using only Theorem I, to show also that $A_{2k\lambda} \equiv B_{2k\lambda-1} \equiv (-1)^{(\lambda(k+1))}$. To prove, however, that $B_{2k\lambda} \equiv M(-1)^{\lambda(k+1)}$ it is necessary to use Theorem II.

Certain interesting palindromic relations develop in the course of the proof, such as

$$A_{2k\lambda-(mk+1)} \equiv (-1)^{(k+1)\,(\lambda+m)}\,A_{mk-1} \pmod{n},$$

with a similar congruence in the $B$'s.

With these values for the convergents of order $2k\lambda$ and $2k\lambda - 1$ it is possible to proceed to those of order $2kn$ and $2kn - 1$ with the results as noted above

$$\begin{aligned} A_{2nk} &\equiv B_{2nk-1} \equiv (-1)^{nk-1} \pmod{n}, \\ A_{2nk-1} &\equiv B_{2nk} \equiv 0 \pmod{n}, \end{aligned}$$

and these last results are, without much difficulty, shown to be valid also for the more general fraction

$$(\overline{a_1, a_2, a_3 \; . \; . \; . \; . \; a_{k-1}, a_k m + b}), \quad m = 1, 2, 3, \; . \; . \; . \; .$$

where $n$ is prime to $a_k$, $A_{k-1}$ and $B_{k-1}$. If we take the still more general fraction with irregular partial quotients:

$$(q_1, q_2, q_3 \; . \; . \; . \; . \; q_r, \overline{a_1, a_2, a_3 \; . \; . \; . \; , a_{k-1}, a_k m + b}), \quad m - 1 \;\; 2 \;\; 3$$

the values given above for the convergents of order $2nk + r$ and $2nk + r - 1$ are no longer correct but must be replaced by the $r$th and $(r - 1)$st convergents of the fraction $(q_1, q_2, q_3 \; . \; . \; . \; . \; q_r)$.

The above mentioned divisibility theorems concerning the Naperian base are easily derived from these general theorems. The special cases where $n$ is not prime to $2a_k$ and to $A_{k-1}$ and to $B_{k-1}$ have been examined in detail. When $A_k$ is divisible by $n$ the fraction reduces modulo $n$ to a purely periodic fraction of $k$ partial quotients with no relations between them. The number of terms in the period of the convergents to such a fraction depends upon the discriminant $D$ of the quadratic equation of which the fraction is one root. For a prime modulus $p$ the period of the convergents is a divisor of $p + (D/p)$ where $(D/p)$ is the symbol of Legendre.

For the case $n$ a divisor of $A_{k-1}$ (or $B_{k-1}$) the period of the convergents is a multiple of $nke$ where $e$ is the exponent to which $A_k$ (or $B_k$) belongs modulo $n$. It is curious that for these special cases the period of the convergents does not seem to be definitely assignable as in the general case.

In this investigation we are concerned only with the successive values of the numerators and denominators of the convergents, and not at all with the existence or non-existence of a limiting value of the convergents. Certain of the fractions involved are closely related to those called 'semi-regular' whose convergence has been studied by Tietze,[4] and the rules which apply to semi-regular continued fractions may be easily modified to apply to them. The research here carried out is really an arithmetical study of the series of numbers which satisfy certain difference equations and, as Professor Birkhoff has remarked, the theorems are thus related to the lesser Fermat theorem and to Wilson's theorem which are concerned respectively with the numbers which satisfy the difference equations $u_{x+1} = au_x$, and $u_{x+1} = xu_x$.

---

[1] Hurwitz, *Zurich, Vierteljahrsch. Natf. Ges.*, **41**, 1896, (34–64).

[2] Cotes, *Phil. Trans., London*, **29**, 1714, (5). (Reference in *Encyc. Sci. Math.*, Paris, Tome I, Vol. I, Fasc. 2, p. 169 note.)

[3] Euler, *Com. Acad. Petropolitanae*, **9**, 1737, (98–137).

[4] Tietze, *Math. Ann., Leipzig*, **70**, 1911, (236–265).

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# ON CLOSED CURVES DESCRIBED BY A SPHERICAL PENDULUM

By Arnold Emch

Department of Mathematics University of Illinois

Communicated by R. S. Woodward, April 18, 1918

1. The geometrical aspect of the problem of closed curves in a spherical pendulum-motion has apparently never been fully discussed and it is the object of this note to present the results of an investigation of some of the geometric properties of these curves.

The differential equations of the motion are

$$\frac{d^2x}{dt^2} = -S\frac{x}{l},\quad \frac{d^2y}{dt^2} = -S\frac{y}{l},\quad \frac{d^2z}{dt^2} = -S\frac{z}{l} - g, \tag{1}$$

in which $l$ is the length of the pendulum, $S$ the variable reaction directed towards the origin (point of suspension). The velocity $v$ of the pendulum-bob is, as is well known, given by $v^2 = h - 2gz$, in which $h$ is a constant depending upon the initial conditions. For $z$ as a function of $t$, we may obtain without difficulty

$$z = \frac{h}{6g} + \frac{2l^2}{g}p\,(t + w_2). \tag{2}$$

In this Weierstrassian $p$-function the one half-period $w_1$ is real; the other $w_2$ is pure imaginary. Putting $u = t + w_2$, let $a$ and $b$ be the arguments for which $z$ assumes the values $-l$ and $+l$, respectively, and $t_1$ and $t_2$ the corresponding complex values of $t$. It is found that $a = i\,\alpha$, where $\alpha$ is real and positive, and $b = w_1 + i\beta$, with $\beta$ equal to a fraction of $|w_2|$. The constant of integration may be determined such that $x = r_0$, $y = 0$, when $t = 0$. Under these conditions $\theta = \tan^{-1}(y/x)$ is defined by

$$e^{2i\theta} = \frac{\sigma(t+t_1)\,\sigma(t+t_2)}{\sigma(t-t_1)\,\sigma(t-t_2)} \cdot e^{-2t\,[\zeta_2(t_1)+\zeta_2(t_2)} \tag{3}$$

From this is found

$$x = \phi(t) = \frac{r_0}{2}\,\frac{\sigma(t+t_1)\,\sigma(t+t_2)\,e^{-t\,[\zeta_2(t_1)+\zeta_2(t_2)]} + \sigma(t-t_1)\,\sigma(t-t_2)\,e^{t\,[\zeta_2(t_1)+\zeta_2(t_2)]}}{\sigma(t_1)\,\sigma(t_2)\,\sigma_2^2(t)}, \tag{4}$$

$$y = x(t) = -\frac{r_2 i}{2}\cdot\frac{\sigma(t+t_1)\,\sigma(t+t_2)\,e^{-t\,[\zeta_2(t_1)+\zeta_2(t_2)]} - \sigma(t-t_1)\,\sigma(t-t_2)\,e^{t\,[\zeta_2(t_1)+\zeta_2(t_2)]}}{\sigma(t_1)\,\sigma(t_2)\,\sigma_2^2(t)}. \tag{5}$$

These solutions may also be obtained from the differential equations.

$$\frac{d^2x}{dt^2} = \left[\,6\,p(t) - \frac{h}{2l^2}\right]x, \tag{6}$$

and a similar equation for $y$. (4) and (5) are uniform analytic functions of $t$, with $\infty$ as the only essential irregularity, and they assume real values for real values of $t$.

Increasing $t$ by $2\,m_1\,w_1$, it is found that the motion will be periodic, when

$$2\,m_1\{\eta_1(a+b) - w_1[\xi(a) + \xi(b)]\} = 2ki\pi, \tag{7}$$

$k$ being a positive integer. When $t$ increases by $2\,w_1$, $\theta$ will increase by the amount

$$\Phi = -\,2i\{\eta_1(a+b) - w_1[\xi(a) + \xi(b)]\}, \tag{8}$$

so that in case of a periodic motion, from (7) and (8),

$$\Phi = 2k\pi/m_1. \tag{9}$$

*The path described by the pendulum is now a closed curve intersecting every level between the lowest and highest position in $2\,m_1$ points and winding $k$ times around the z-axis, before it closes.*

Moreover a function-theoretic investigation of the periodic function

$$F(t) = \alpha.\phi(t) + \beta.\psi(t) + \gamma, \tag{10}$$

whose zeros determine the intersections of the straight line $\alpha x + \beta y + \gamma = 0$ with the horizontal projection of the curve $(x, y)$, shows that *the curve is algebraic and passes through the circular (isotropic) points at infinity (i.e., its horizontal projection).* From (3) follows that *the curve has an $m_1$-fold axial symmetry.*

2. In Greenhill's case[1] the pendulum-bob reaches (but does not go above) the horizontal plane of suspension with a non-vanishing horizontal velocity, and the parametric expressions for the horizontal projection may be written in the form:[2]

$$x = r_0[\cos (A + \pi)\, v.\ \mathrm{cn}\, (2\, Kv) - \mu \sin (A + \pi)\, v.\ \mathrm{sn}\, (2\, Kv)\, \mathrm{dn}\, (2\, Kv)\, ], \quad (11)$$

$$y = r_0[\sin\, (A + \pi)\, v.\ \mathrm{cn}\, (2\, Kv) + \mu \cos\, (A + \pi)\, v.\ \mathrm{sn}\, (2\, Kv)\, \mathrm{dn}\, (2\, Kv)]. \quad (12)$$

If $x' = r_0\ \mathrm{cn}\ (2\ Kv),\ y' = \mu r_0\ \mathrm{sn}\ (2\ Kv)\ \mathrm{dn}\ (2\ Kv),\ z' = a\ \mathrm{cn}^2\ (2\ Kv),$ (13)

it is found that *the locus of the point* $P'(x', y', z')$ *is a quartic* $C_4$ *in space, and that* $P'$ *is obtained by rotating the point* $P\ (x, y, z)$ *of the pendulum curve* $C$ *about the z-axis through an angle*$-(A + \pi)\, v$, *i.e., through an angle negatively proportional to the time associated with* $P$ *in the motion.*

Imposing upon (11) and (12) the same condition of periodicity, as in the general case, *the resulting curve in the xy-plane becomes a rational algebraic curve of order* $2\ k$. *The angle corresponding to the period* $2\ w_1$ *is, as before,* $\Phi = 2k\pi/m_1$; $k$ *and* $m_1$ *being relatively prime. For a given odd* $m_1$ *there are* $(m_1 - 1)/2$ *curves of this type, for an even* $m_1$, *there are* $(m_1 - 2)/2$ *such curves. In both cases there are* $m_1(k - 1)$ *real double points.*

*When* $m_1$ *is odd there is just one curve among the set whose double-points are all real. Its degree is* $2\ k = m_1 + 1$.

When $m_1$ is even, there is no such curve.

In case of an odd $m_1$ the polar equation of the curve has the form

$$\begin{aligned} &\rho^{2k} + a_1\rho^{2k-2} + a_2\rho^{2k-4} + \ .\ .\ .\ .\ + a_j\rho^2 + a + \\ &(b_1\rho^{2\lambda_1} + b_2\rho^{2\lambda_2} + \ .\ .\ .\ .\ + b_j\rho^2 + b)\ \rho^{m_1} \cos m_1\theta = 0, \end{aligned} \quad (14)$$

in which $a \pm 0$, $\lambda_1 > \lambda_2 > \lambda_3 > \ \ldots\ > 1$, and $2\ \lambda_1 + m_1 \leqq 2\ k - 1$. In cartesian coordinates (14) may be written in the form

$$\begin{aligned} &(x^2 + y^2)^k + a_1\ (x^2 + y^2)^{k-1} + a_2\ (x^2 + y^2)^{k-2} + \ .\ .\ .\ .\ + a_j\ (x^2 + y_2) + a + \\ &\qquad \{b_1\ (x^2 + y^2)^{\lambda_1} + b_2\ (x^2 + y^2)^{\lambda_2} + \ .\ .\ .\ .\ + b_j\ (x^2 + y^2) + b\}. \quad (15) \\ &\left\{x^{m_1} - \binom{m_1}{2} x^{m_1-2} y^2 + \binom{m_1}{4} x^{m-4} y^4 + \ .\ .\ .\ .\ + (-1)^{\frac{m_1-1}{2}} \cdot xy^{m_1-1}\right\} = 0. \end{aligned}$$

Transforming this curve by

$$x = \pm \sqrt{x'' - y''^2 - 2y'' - 1}\ /\ y'',\ y = (y'' + 1)\ /\ y'',$$

or using isotropic coordinates, transformations which do not change the character of the isotropic points, and "placing the curve on the analytic triangle," the result is obtained that the isotropic points absorb together

$$(k - 1)\ (2\ k - m_1 - 1) \quad (16)$$

double points, which when added to the $m_1\ (k - 1)$ real double-points, gives $(k - 1)\ (2\ k - 1)$, i.e., the maximum number of double points which a curve of order $2\ k$ may have. Thus we have verified directly from the equation, that the curve is rational, as proved before. When the curve has all double points real, then its degree is $m_1 + 1 = 2\ k$, so that from (16) the number of imaginary double points is clearly zero, which is another verification of previous results. (16) is also true when $m_1$ is even. Polar and cartesian equations for $m_1$ even may be established in a similar manner as in (14) and (15).

3. Examples.—When $m_1 = 3$, $2k = 4$, the polar and rational parametric equations of the curve[3] are

$$\rho^4 - R\rho^3 \cos 3\theta - \frac{27}{4} R^2\rho^2 + \frac{27}{4} R^4 = 0$$

$$x = \frac{t^4 - 12t^2 + 3}{4\,(1 + t^2)^2},\ y = \frac{2t\,(3 - 5t^2)}{4\,(1 + t^2)^2};\ (R = \tfrac{1}{4})$$

When $m_1 = 4$, $2k = 6$, cartesian and polar equations of the curve are

$$3(x^2 + y^2)^3 - 24(x^4 + y^4) - 32\ x^2y^2 + 39(x^2 + y^2) - 18 = 0,$$
$$3\,\rho^6 - (2 \cos 4\theta + 22)\rho^4 + 39\rho^2 - 18 = 0.$$

As $m$ and $k$ increase, the construction of the equations with numerical coefficients becomes increasingly difficult.[4]

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[1] Greenhill, *Les Fonctions Elliptiques et leurs Applications*, chap. III.

[2] Tannery et Molk, *Elements de la Théorie des Fonctions Elliptiques*, **4**, pp. 176–192; Appell: *Traité de Mécanique Rationnelle*, **1**, p. 494.

[3] This curve is known and was investigated by G. de Longchamps: *J. Math. Elémentaires*, **4**, 1885, 269–277; also by H. Brocard: *J. Math. Spéciales*, (Ser. 3), **5**, 1891, 56–64.

[4] A curve of this type may be symbolically denoted by $C_{2k}^{m_1}$. Tabulations of all curves, with their real and imaginary double points as far as $C_{22}^{12}$ have been made, also actual graphs of $C_4^3$, $C_4^6$, $C_6^5$. $C_8^5$.

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# *THE TAXONOMIC POSITION OF THE GENUS ACTINOMYCES*

By Charles Drechsler

Cryptogamic Laboratories, Harvard University

Communicated by R. Thaxter, May 14, 1918

To the genus Actinomyces are usually assigned a variety of peculiar and very minute filamentous organisms, widely distributed in nature, concerning the taxonomic relationship of which divergent views have been held. Most of the earlier medical writers, whose attention was centered on forms associated with human and animal diseases, placed the genus with the pleomorphic bacteria. Others recognized conidia in the clavate elements produced by one of these pathogenic forms, *Act. bovis*, within the animal body, and referred this parasite to the Fungi. These elements were later shown to represent degenerative structures; but as subsequent investigations on a number of species, including various saprophytic forms, as well as the common potato scab organism and several pathogenic types clinically similar to *Act. bovis*, revealed decidedly fungoid characteristics, not only in the profusely branched condition of the vegetative thallus when grown in culture, but also in the production of an aerial mycelium and Oidium-like spores, the view that the genus is to be placed with the Hyphomycetes has continued to receive support.

According to another view which has gained quite wide acceptance, Actinomyces represents an intermediate group and a phylogenetic connection between the Bacteria and the Fungi. It is held that the genus originated either as the result of the development of bacterial forms possessing a tendency toward the branched condition, like the tubercle and the diphtheria organisms; or as the result of the reduction of hyphomycetous types, which, in their ultimate stages, yielded the much simpler true bacteria.

In order to determine the merits of these contending views, the writer subjected a large number of saprophytic species isolated from the soil, air, etc., as well as several virulent strains of the potato scab organism secured from Mr. M. Shapavalov, to morphological study. The results may be summarized as follows:

(1) The vegetative thallus of Actinomyces consists of a mycelium composed of profusely branching hyphae, the terminal growing portions of which are densely filled with protoplasm. Toward the center of the thallus, the vacuoles increase in size, and may be associated with the presence of metachromatic granules; the latter having nothing in common with bacterial endospores or 'micrococci,' for which they were mistaken by early observers.

(2) The vegetative mycelium attains an extent incomparably greater than the branching figures recorded for bacteria of the acid-fast group; and the hyphae lack the uniformity in diameter generally characteristic of the Schizomycetes.

(3) The aerial mycelium produced on suitable substrata by most species, occurs, usually, in the form of a mat of discrete fructifications; but in other species, these fructifications are frequently combined to form numerous peculiar erect Isaroid sporodochia.

(4) In any case, each individual fructification represents a well characterized sporogenous apparatus, consisting of a sterile axial filament bearing branches in an open racemose, or dense capitate arrangement. The primary branches may function directly as sporogenous hyphae, or may proliferate branches of the second and of higher orders; sporogenesis, in the latter case, being confined to the terminal elements, the hyphal portions below points of attachment of branches remaining sterile.

(5) Two tendencies in the development of fructifications are recognizable, one leading to an erect dendroidal type in which successively proliferated fertile elements undergo processes of sporogenesis in continuous sequence; and the other leading to a prostrate, racemose type, in which sporogenesis is delayed in the older branches until the younger branches have also attained their

## ERRATA

Due to printer's error the figures upon pages 225-228 were printed in the August, 1918, issue in incomplete form.

When binding the volume substitute the pages herewith for those in the August, 1918, issue.

final extension. The majority of species show these tendencies combined in different ways.

(6) The sporogenous hyphae of most species are coiled in peculiar spirals sometimes resembling the spores of the hyphomycetous genus Helicoön. These spirals exhibit pronounced specific characteristics in the number, diameter, and obliquity of their turns, and especially in the direction of rotation (whether dextrorse or sinistrorse).

(7) Sporogenesis, where it can be followed, begins at the tips of the fertile branches and proceeds basipetally. In the larger number of species the process involves the insertion of septa, which in certain cases, are relatively very massive, and in others, so thin as to be barely discernible. The disposition of these septa while the delimited spores undergo maturation processes, varies with the species: (1) they may remain more or less unaltered; (2) they may suffer a median split, the two resulting halves being then separated as the result of the longitudinal contraction of the young spores, leaving alternate portions of hyphal wall completely evacuated; (3) or they may first become considerably constricted and subsequently converted into non-stainable isthmuses connecting the mature spores. The apparent absence of septa in the sporogenous hyphae of other forms, is, perhaps, attributable to optical difficulties.

(8) Granules are readily differentiated in the spores of many species, which possess the staining properties and uniformity of size characteristic of nuclei; they generally occur singly, but in the larger spores of a few forms, two are often found occupying diagonally opposite positions.

(9) As in the vegetative thallus, metachromatic granules occur in the aerial mycelium, being very rarely found in spores or sporogeneous hyphae but becoming very abundant in degenerate sterile hyphae.

(10) The older axial filaments of some species show marked distensions, which, in extreme cases, result in figures simulating Leptomitus. These arise as local distensions at the points of attachment of the more extensive lateral sporogenous processes. Cuneate modifications of the sterile axial filaments below the origin of branches, also occur.

(11) Curious spherical structures appear regularly in some forms, both in the sterile axial hyphae, where they may contain either a median septum or a number of peripheral metachromatic granules, and in the sporogenous hyphae where they are associated with the regularly spaced septa.

(12) The spores germinate readily in suitable solutions, producing from one to four germ tubes, the approximate number being more or less characteristic of the species.

(13) Owing to the absence of any well defined bacterial characteristics, the writer is of the opinion that the view that Actinomyces represents a transition between the Hyphomycetes and the Schizomycetes, as well as the phylogenetic corollary based upon it, may safely be abandoned. If mere size is to be regarded as important, it would appear to be equally profitable to look

for bacterial affinities in some ascomycetous and sphaeropsidaceous forms, the hyphae of which are similarly very minute. It is doubtful whether far-reaching taxonomic generalizations can be based upon the 'acid-fast' staining reaction, especially as this reaction has not played a very important rôle in mycological research. There seems to be no adequate reason why the genus should not be classed, in an unqualified manner, with the Hyphomycetes, as a Mucedineous group with tendencies toward an erect Isaroid habit.

A more complete illustrated account will appear shortly in the *Botanical Gazette.*

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## *STUDIES OF MAGNITUDES IN STAR CLUSTERS, VIII. A SUMMARY OF RESULTS BEARING ON THE STRUCTURE OF THE SIDEREAL UNIVERSE*

By Harlow Shapley

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Communicated by W. S. Adams, May 21, 1918

In the preceding communication of this series[1] methods were discussed for the determination of the relative distances of a considerable number of globular clusters. The methods have now been developed so as to give not only relative values but also fairly reliable absolute distances for all globular clusters, and for all variable stars of the Cepheid class for which periods and apparent magnitudes are known. A rather detailed summary of the procedure, its accuracy, and the results of a thorough application of the methods, has been given in the February issue of the *Publications of the Astronomical Society of the Pacific.* The present note will be confined to a synopsis of the more important results pertaining to the probable extent and arrangement of the sidereal system. The detailed discussion is appearing in a series of papers in the *Astrophysical Journal*, and will be separately published as *Contributions from the Mount Wilson Solar Observatory*, Nos. 151–157.

Extending to the globular clusters the work of Miss Leavitt, Hertzsprung, and Russell on the Cepheid variables of the Small Magellanic Cloud and of the galactic system, we have been able to establish beyond question the interdependence for these variables of absolute luminosity and period of light variation. By combining the apparent and absolute magnitudes, the distances and positions in space have been determined for about 140 Cepheid variables, most of which are much more distant than any objects for which parallaxes have been directly measured. Figure 1 shows their distribution.

The distances of globular clusters are of a different order of magnitude from those heretofore entering stellar investigations. Although the average naked-eye star is near as compared with many Cepheid variables, the most remote Cepheid now known is not so far away as the nearest globular cluster. The available astronomical records contain 69 clusters that appear definitely to belong to the globular classification. Further work on very faint and distant objects will probably add a few to the present list, but within a distance of 100,000 light-years of the sun the survey appears to be complete. Keeping this limitation in mind, we may examine the collected data for signs of a general organization.

The apparent concentration of the globular systems to a southern region of the Milky Way has long been known. It now appears, upon closer investigation, that few if any typical globular clusters are to be found within 5° of the galactic plane; and, when actual positions in space are substituted for apparent positions, this suggested avoidance of the mid-galactic region reveals itself as a total absence of compact clusters from the domains of space that appear to contain most of the known sidereal bodies.

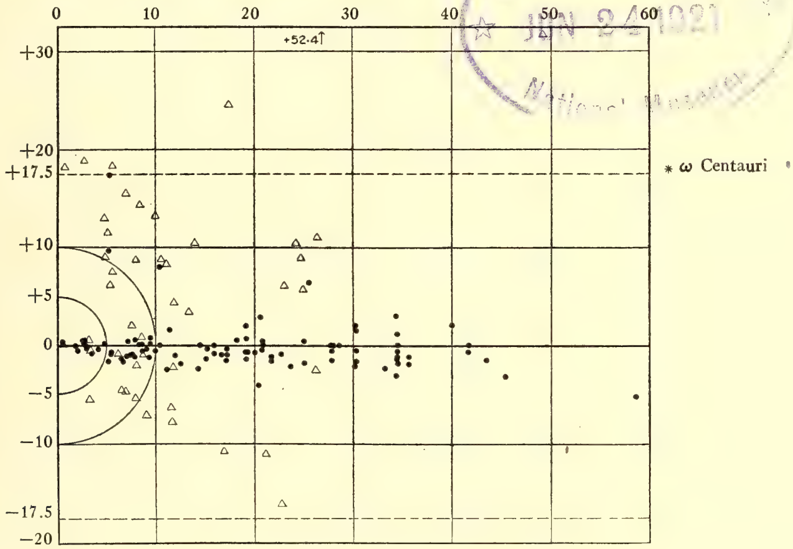


FIG. 1. DISTRIBUTION OF CEPHEID VARIABLES

The unit of distance is 100 parsecs. Ordinates are distances from the galactic plane; abscissae are projected distances in the plane. Open triangles and black dots designate, respectively, cluster-type variables and Cepheids with periods in excess of a day. The nearest globular cluster, ω Centauri, is just outside the boundary of the diagram on the right. RU Boötis, indicated by an arrow, is too far above the plane to fall within the figure. The semicircles, with radii of 500 and 1000 parsecs ($\pi = 0''.002$ and $0''.001$), indicate how distant most of these variables are as compared with the average star of the tenth magnitude or brighter ($\pi > 0''.004$, Kapteyn). Between the broken horizontal lines, ±1750 parsecs, lies the equatorial galactic region devoid of globular clusters.

The striking distribution of globular clusters in galactic longitude is well shown in the projection of their positions on the galactic plane in figure 2. Apparently the clusters themselves form a large flattened system, the center of which, in the galactic plane, is between 60 and 70 thousand light-years distant in the general direction of the dense star clouds of Sagittarius and Scorpio.

The projection of the positions of clusters on a plane perpendicular to the Milky Way, and parallel to the direction of this center from the sun, is illustrated in figure 3. The shaded portion of the diagram indicates the equatorial region, toward which globular clusters crowd but in which they are not found; its thickness appears to be only three or four per cent of its extent along the galactic plane.

There can be little doubt that the galactic plane defined by the faint stars and by the Milky Way clouds is also a symmetrical and fundamental plane for the system of globular clusters. In other words, the distribution of clusters shows that, notwithstanding their great dimensions, they are subordinate members of the far greater galactic system. Their arrangement and the relative densities of various parts of the Milky Way clouds strongly suggest that the whole sidereal system is roughly outlined by the positions of globular clusters, and that all known celestial objects—stars, nebulae, clusters—are members of a single unit.

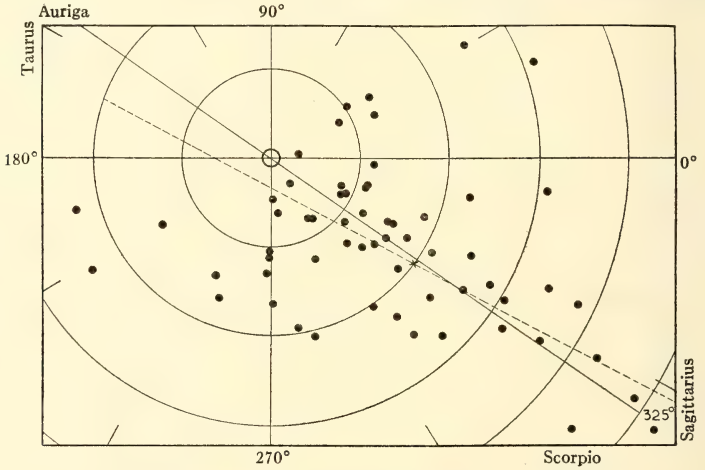


FIG. 2. THE SYSTEM OF GLOBULAR CLUSTERS PROJECTED ON THE PLANE OF THE GALAXY

The galactic longitude is indicated in the margin. The 'local system' is completely within the smallest circle, which has a radius of a thousand parsecs (3260 light-years). The larger circles, which are also heliocentric, have radii increasing by intervals of 10,000 parsecs. The dotted line indicates the suggested major axis of the system (if ellipsoidal), and the cross the adopted center. The dots are about five times the actual diameters of the clusters on this scale. Nine clusters more distant from the plane than 15,000 parsecs are not included in this diagram.

The mean diameter of the proposed system appears to be at least 300,000 light-years; its most conspicuous feature is the equatorial segment, which apparently is thickly populated with stars throughout its whole extent. From this viewpoint the Milky Way is mainly a phenomenon of depth; its extent, as seen from the sun, is something like three times as great in the direction of the center as in the opposite direction toward Auriga and Taurus. The testimony of the star frequencies in the Milky Way clouds does not disagree with the supposition of a remarkably eccentric position of the solar system.

Slipher's radial velocity observations of the brighter globular clusters[2] indicate that seven out of eight of those with high galactic latitudes are approaching the sun (and probably the equatorial segment) with such high velocities that, unless greatly retarded, they will have entered the dense stellar regions within an interval of time which appears to be short as compared with the probable history of a stellar system. The absence of such clusters from the Galaxy thus becomes the more remarkable. The globular systems nearest the galactic plane are in general the least condensed. This result and the distribution of stars in certain open clusters suggest the possibility that upon approaching the galactic regions globular clusters may be disrupted and transformed into open galactic groups.

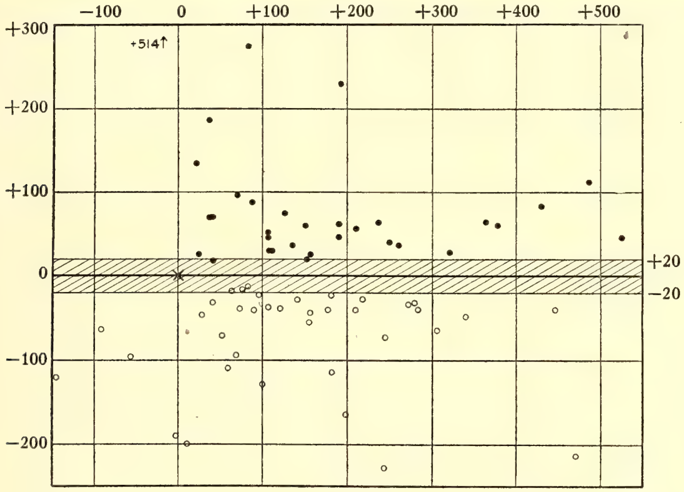
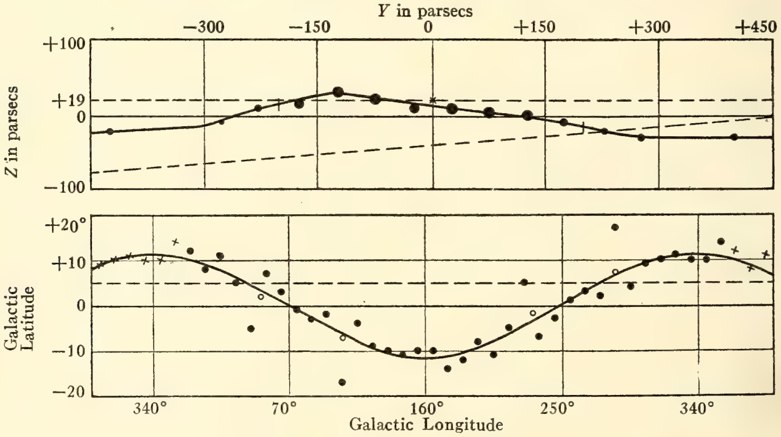


FIG. 3. PROJECTION OF THE POSITIONS OF GLOBULAR CLUSTERS ON A PLANE PERPENDICULAR TO THE GALAXY

Illustrating (1) the absence of clusters from the mid-galactic region, (2) their symmetrical arrangement with respect to the Galaxy, (3) the eccentric position of the sun (the cross) with respect to the center of the system of clusters. The ordinates are distances from the galactic plane, $R \sin \beta$; the abscissae are projected distances in the direction of the center, $R \cos \beta \cos (\lambda - 325°)$. The unit of distance is 100 parsecs (326 light-years); each small square is 10,000 parsecs on a side. On this scale the actual diameter of each cluster is about one-fifth the diameter of the circles and dots. The cluster N.G.C. 4147 is outside the boundary of the diagram, as indicated by the arrow.

The galactic center as derived from the clusters is nearly at right angles to the direction of center obtained by Walkey and Charlier from statistical investigations of the brighter stars. The stars of spectral type B, according to Charlier,[3] form a flattened system of some 2000 light-years radius, which he identifies with the general stellar universe; the group is very small, however, as compared with the system now outlined by globular clusters, and we may assume instead that these B stars comprise a localized stellar organization.

To test further for the existence of a limited local cluster, situated far within the bounds of the equatorial segment and perhaps comparable in some respects with other open galactic groups, an investigation has been made of the galactic arrangement of the brighter stars. Details of this study are given in *Mount Wilson Contribution* No. 157. In brief, a verification is obtained of the presence of a local cluster, for which the following properties are indicated: (a) it contains very nearly all the B stars brighter than the seventh magnitude (the remainder appearing to be members of the intermingling and surrounding galactic field), a majority of the A stars, and large numbers of those of redder spectral types; (b) its central plane is not more than 30 light-years south of the sun and may be much nearer; on the other hand the true galactic plane, as defined by Cepheid variables, faint stars, and the galactic clouds, is approximately 175 light-years south of the center of the local cluster (fig. 4*a*), (c) the central plane, at least for the brighter stars, in inclined about 12° to



FIGS. 4*a* AND 4*b*

Fig. 4*a*. (Above) Projection of the local cluster of B stars on a plane perpendicular to the Galaxy,—adapted from the data shown in Plate IV of Charlier's memoir. The inclination of the cluster's central plane to the Galaxy has been partially eliminated by Charlier. The projected center of the system of B stars, as derived by him, is at the origin of co-ordinates; the present work suggests that the central plane of the B stars is much nearer the sun (whose position is indicated by the cross) than Charlier supposed. The projection of the true galactic plane appears as a broken inclined line, and the distance of the sun and the local cluster north of it is to be noted. Short vertical lines across the curve show the limits of distance adopted for the solution represented by Fig 4*b*.

Fig. 4*b*. (Below.) Solution for the inclination of the local cluster to the galactic plane, based upon 400 stars of spectral-type B. Owing to the sun's position to the north, the central plane has a dip of 5°.

the galactic plane, with nodes in longitude 70° and 250° (figs. 4*a* and 4*b*); (d) the diameter is of the order of 2500 light-years.

The above results lead to a simple interpretation of star-streaming. The motion of an open cluster through the general star-fields of the equatorial segment must give rise to stellar drifts, and it is a natural assumption that the observed streaming in the neighborhood of the sun is wholly due to such a cause. According to this view, stars of Stream I belong to the large moving cluster surrounding the sun; those of Stream II belong to the galactic field. The motion of the cluster as a whole is in the galactic plane, nearly radial from the galactic center, and there is considerable evidence of internal motion within the cluster. In all the details examined, this hypothesis appears to be in agreement with the observed systematic motions of the stars.

[1] Shapley, H., these PROCEEDINGS, **3,** 1917, (479–484); *Mt. Wilson Communication,* No. 37.

[2] Slipher, V. M., *Popular Astronomy, Northfield, Minn.,* **26,** 1918, (8).

[3] Charlier, C. V. L., *Meddelanden fran Lunds Astronomiska Observatorium, Upsala,* (Ser. 2), No. 14, 1916, (1–108).

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# *GLACIAL DEPRESSION AND POST-GLACIAL UPLIFT OF NORTHEASTERN AMERICA*

By H. L. FAIRCHILD

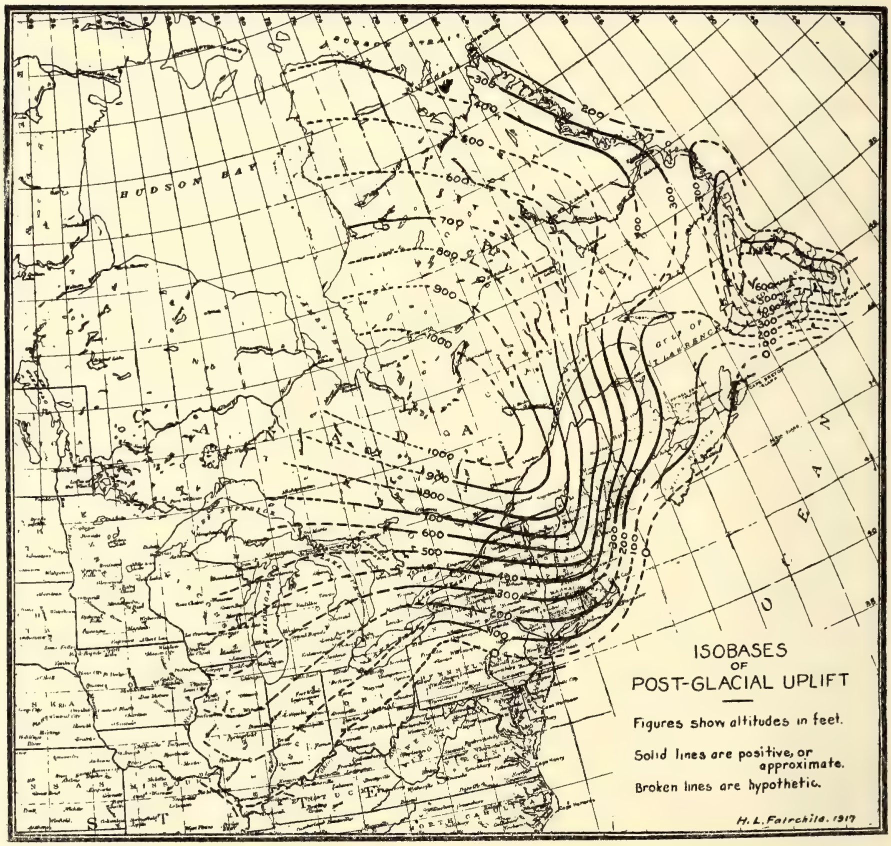
DEPARTMENT OF GEOLOGY, UNIVERSITY OF ROCHESTER

Communicated by J. M. Clarke, June 4, 1918

The geophysical theory of isostacy is excellently illustrated by the up and down (diastrophic) movements of northeastern America in relation to glaciation. The amount and the area of land depression beneath the ice sheet, and the land uplift subsequent to the removal of the ice, is fairly proportionate to the thickness and extent of the latest ice cap.

The fact is evident that the area covered by the latest continental ice sheet, the Labrador (Quebec) glacier, stood much beneath its present altitude, relative to sea-level, when the ice melted off; and that the recent uplift has brought the land to its present position. The evidence of the uplift is abundant; many high-level beaches and sand-plains facing the open sea and extending far up the valleys in Canada, New England and New York, with the occurrence of abundant marine fossils hundreds of feet above the ocean. These facts have been recognized for quite a century, and many observations are recorded in the geologic literature of Canada and America. But up to the present time the full amount of submergence and the extent or limits of the drowned area have not been determined beyond dispute. The full amount of the down-and-up movement has nearly always been underestimated, because the conspicuous or more evident marine features are generally of inferior and later water levels, while the initial or summit features are commonly weak and unobtrusive; or the latter lie so far inland and are so detached as to be unrecognized in their genesis and relationship, being commonly referred to glacial origin. However, the summit or initial level at any point is the critical and essential element in the problem.

Several years of study in the Hudson-Champlain, Ontario and Connecticut valleys has determined the position of the uplifted and tilted marine level.


HUDSON BAY
CANADA
GULF OF ST. LAWRENCE
OCEAN
ISOBASES OF POST-GLACIAL UPLIFT
Figures show altitudes in feet.
Solid lines are positive, or approximate.
Broken lines are hypothetic.
H. L. Fairchild, 1917
1000
900
800
700
600
500
400
300
200
100
0

In the Hudson-Champlain Valley the ancient estuary features rise from zero south of New York City to 740 feet on the north boundary of New York, and to over 800 feet on the north line of Vermont. Comparison of these features with similar phenomena in the Connecticut Valley gives the direction of the isobases (lines of equal uplift) as 20 degrees north of west by south of east; or 20 degrees east of north for the direction of steepest tilting. This figure is in close agreement with the determinations of Coleman, Goldthwait, Spencer and Taylor for the later deformation of the glacial lake shore lines in the Great Lakes area.

It has also been found that in the Ontario Basin the vertical interval between the tilted plane of Lake Iroquois and the plane of the sea-level waters (Gilbert Gulf) is 290 feet. Extension of the isobases of total uplift westward over the Ontario Basin gives very close accordance with the facts of observation in that field.

With the large area of New York State, Ontario basin and western New England as a well-determined base it has been possible to extend the study eastward over New England and eastern Canada. The result is shown in the accompanying map with at least close approximation to accuracy. On the small scale the map is somewhat generalized. The broken lines are entirely hypothetic only in the Mississippi Valley, where the land uplift may be more complicated in time and form. Except in the district west of Indiana and Michigan the map shows the rise of the continent subsequent to the removal of the latest ice sheet.

For Laborador and Newfoundland reliance is placed on the published data of R. A. Daly, with some help from unpublished figures of A. P. Coleman and J. B. Tyrrell.

Precaution is taken in this study to discriminate between features produced by sea-level waters and by glacial waters. In the inland areas, in order to avoid doubt or cavil as to glacial waters, the main dependance has been placed on the summit deltas of streams with southward flow, or with flow directed away from the receding ice margin. In the extended paper, noted below, will be found a description of field methods, and discussion of criteria for discriminating marine features.

The map shows apparently direct relation between the ice sheet and the diastrophic land movement. The area of uplift is the area of glaciation, and the amount of uplift is proportionate to the supposed thickness of the spreading ice cap. The map also shows the effect of land and sea on the flow and reach of the ice sheet. The ice deployed on the land but was inhibited by the sea; thus producing more rapid flow and steeper gradients along the radii toward the nearer sea shores. An independent ice cap is indicated for Newfoundland.

For the fuller discussion, in both methods and results; for description of the uplifted sea-level features in western New England, Maine, St. Lawrence and Ottawa valleys, Gaspé peninsula, New Brunswick, Nova Scotia, Labrador and Newfoundland; and for discussion of possible effects of any change in ocean level, the reader is referred to the formal paper in the *Bulletin of the Geological Society of America*, volume 29.

Former papers by the writer bearing on the subject of recent land uplift are as follows:

Pleistocene marine submergence of the Connecticut and Hudson Valleys, *Bull. Geol. Soc. Amer., New York*, **25**, 1914, (219–242).

Pleistocene uplift of New York and adjacent territory, *Ibid.*, **27**, 1916, (235–262).

Post-Glacial marine waters in Vermont, *Burlington, Rep. Vermont State Geologist for* 1915–16, **1917**, (1–41).

Poat-Glacial submergence of Long Island, *Bull. Geol. Soc. Amer.* **28,** 1917, (279–309).
Post-Glacial features of the upper Hudson Valley, *N. Y. State Museum, Bull.*, 195, 1917.
Post-Glacial uplift of northezastern America *Bull. Geol. Soc. Amer.*, **29** (in press), 1918.

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# *A BACTERIOLOGICAL STUDY OF THE SOIL OF LOGGERHEAD KEY, TORTUGAS, FLORIDA*

By C. B. Lipman and D. D. Waynick

College of Agriculture, University of California

Communicated by A. G. Mayer, May 21, 1918

Inasmuch as the coarse calcareous sands of the islands off the Florida Coast, and of similar ones, represent very recent geological material, and since they, therefore, offer an opportunity of determining the early bacterial flora which establish themselves there, it was decided to carry out some studies on typical samples. Dr. A. G. Mayer, Director of the Marine Biological Laboratory of the Carnegie Institution, situated on Loggerhead Key, Tortugas, Florida, supplied us with the necessary samples for our study. Three large samples were collected, which answer to the following descriptions, for which we are indebted to Dr. Mayer:

No. 1. In region thickly covered with *Suriana maritima* bushes. About twenty feet north of stone wall built in 1868 and in a place where probably no man has trodden for 30 years or more. This sample is of an average depth of about 7 inches beneath the surface.

No. 2. Sand from the surface to 6 inches in depth from the northern end of Loggerhead Key. The region is barren of vegetation, no plants having ever grown within 200 feet of the place from which sample was taken. It is about 6 feet above high tide level on the crest of the island. Probably no man has walked here for 10 months previously.

No. 3. From an average depth of 15 inches below the surface in a place densely wooded with *Suriana maritima.* Same locality as Sample No. 1.

We thus had a soil and a subsoil sample from a part of the island in which large bushes (*Suriana maritima*) have established themselves as a permanent association. We also had a surface sample of very coarse, white, calcareous sand or grits, on which plants have never grown. It is to be noted, also, that there has been little or no opportunity for the contamination of these samples by the habitation or tread of man. The flora which now characterize the soil or sand material must take their origin either from the sea water, which now surrounds and which at one time probably covered them, or from winds carrying dust from older soils. The samples were collected by Dr. Mayer with the greatest care, large sterile bottles with cotton stoppers having been employed as containers. The cotton stoppers were doubly protected against contamination while in transit.

The studies carried out included counts of bacteria in the various samples, isolation and identification of pure cultures of the important bacteria and fungi growing on mannite, beef, and synthetic agar, the determination of the soils ammonia and nitrate producing powers, of their nitrogen fixing powers, and of the isolation and study of the nitrogen fixing organisms found. In this preliminary note, we give merely a brief general statement anent our findings and reserve for another paper the more detailed discussion of the results obtained.

*Bacterial Counts.*—In the surface soil from the wooded part of the island, as many as a million organisms per gram were found when beef agar was used as a medium. Less than half as many organisms developed on synthetic agar and only about $\frac{1}{15}$ as many on mannite agar. In the subsoil from the same spot, there were approximately $\frac{1}{15}$ as many organisms developing on beef agar as on the surface soil and of synthetic agar the corresponding number was about $\frac{1}{10}$ that of the surface soil on beef agar and less than $\frac{1}{4}$ that on synthetic agar. The subsoil developed only one organism per gram on mannite agar.

In the highly calcareous sand free from vegetation, there were only about 8000 to 9000 organisms per gram of material on both beef and synthetic agar and only $\frac{1}{10}$ as many on mannite agar.

*Nitrogen Transforming Powers of the Soils.*—Both soil and subsoil from the wooded part of the island show powers of producing ammonia from dried blood nitrogen about equal to those of a poor sandy soil. The calcareous sand which, as has been observed above, contains relatively few organisms, possesses, nevertheless, a power of producing ammonia about three-quarters as great as that of the other soil material.

As regards nitrifying power, all the soils seem to be very feeble, if indeed they possess any such power appreciably. The amount of nitrate produced by the subsoil of the wooded part of the island seems to be above the limit of error, but, curiously enough, the surface soil produces no nitrate from sulphate of ammonia. The calcareous sand appears to produce a small quantity of nitrate from sulphate of ammonia, but the amount so formed may be within the limit of error. From the small amount of nitrogen which the soil itself contains, which in no case attains 0.01%, all of the samples seem to be powerless to produce nitrate. Dried blood in the quantities used seems to be no more satisfactory than sulphate of ammonia. These facts make possible some interesting speculation as to the nitrogen nutrition of the plants growing on the island, which we shall discuss in a future paper.

*Nitrogen Fixing Powers and Organisms.*—Perhaps the most interesting results obtained in these studies were those on the nitrogen fixing powers and organisms of the soil materials in question. All the samples gave characteristic Azotobacter films in mannite solution, the surface soil from the wooded land giving the heaviest film and the characteristic deep black pigment formation usually ascribed to *A. chroococcum.* The subsoil from the wooded land and the calcareous sand produced thin discontinuous films and little or no pigment. All the films, on microscopic examination, showed typical Azotobacter cells. Judging, therefore, from the very small number of organisms which are found in the calcareous sand, Azotobacter, a nitrogen fixing organism, seems to be one of the earliest and one of the most numerous organisms. The nitrogen fixing power of the soils as measured by the ordinary laboratory test in solution cultures, in contrast with their nitrogen transforming powers, seem to be as vigorous as those of excellent soils. It is interesting, moreover, that the calcareous sand fixed about $\frac{5}{8}$ as much nitrogen in the tests mentioned as the surface soil from the wooded land and about $\frac{5}{8}$ as much as the subsoil of the same land.

Space does not permit a consideration here of some of the pure cultures of bacteria and fungi which were isolated from the soil samples studied. Three species of Actinomyces found appear to be new and as yet remain unnamed. Some of the common organisms of all soils were found, including bacteria, *Actinomyces*, and fungi. These will all be described in detail in a forthcoming paper, mention of which has already been made above.

It is a privilege to acknowledge again our obligation to Dr. A. G. Mayer for his kindness in sending the samples and for his interest in the work. We also express thanks to Dr. H. J. Conn and to Dr. S. A. Waksman for assisting in identification of a few cultures of bacteria and of *Actinomyces*, respectively.

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# *AUTONOMOUS RESPONSES OF THE LABIAL PALPS OF ANODONTA*[1]

By P. H. Cobb

Zoölogical Laboratory, Harvard University

Communicated by G. H. Parker, June 19, 1918

Although the ciliary responses of the labial palps of pelecypods have been much studied, the muscular movements of these organs have been entirely neglected. If one valve of an Anodonta is cautiously chipped off leaving the subjacent mantle-lobe intact and the animal resting in the opposite valve, the mantle-lobe thus freed may be folded back so as to expose the parts of the animal lying within the mantle chamber. In this way the labial palps in an almost undisturbed condition may be exposed and worked upon.

In such a preparation the external palp is to be seen resting on the internal one and both are quite flat. If, now, the external palp is touched with a blunt pointed instrument, particularly in its mid-dorsal region, the organ quickly buckles in on its dorsal edge close to its attachment to the mantle and soon after begins to curl from its free tip toward its attached base. On stimulating the internal palp, it responds as the external one does. Both palps in responding curl away from their opposed faces. The vigor of their response is apparently proportional to the stimulus. Grains of sand dropped on the outer face of the external palp affect it as a slight mechanical stimulus, which calls forth some curling but almost no buckling. Currents of water when driven against the palp have very little effect as stimuli unless they are strong enough to indent the palp.

The palps are also open to chemical stimulation and the responses thus called forth are commonly much more pronounced than those due to mechanical stimulation. The chemical stimuli employed consisted in solutions of a number of common salts, acids, and alkalis, of such non-electrolytes as ethyl alcohol, sugar, urea, quinine and so forth, and of mixtures such as beef extract and the like. These were used in varying concentrations and to all, except sugar, a reaction much like that seen in vigorous mechanical stimulation was observed. In reaction to chemical stimulation, however, the palp showed a tendency to roll up tightly from the tip rather than simply to curl upon itself.

Electricity is also a stimulus for the palp. A faradic current just strong enough to be slightly stinging to the human tongue caused the palp to respond as to a mechanical stimulus. In efficiency the electrical stimulus was apparently midway between the mechanical and the chemical stimuli.

A jet of hot water that emerged from the container at 54°C. caused the palp to curl vigorously when it was directed on that organ in water at 16.5°C. A similar jet of water at the temperature of that in which the clam was, had no stimulating effect on the palp.

A beam of sunlight, or of strong electric light, or a sudden burst of light from flash-light powder had the remarkable property of causing the palp to curl. The response, which of course occurred well under water, though slower in its appearance than the responses to other forms of stimuli, followed so quickly on the stimulus that there was no doubt that the light, and not some accompanying disturbance, was the effective agency.

The surprising feature in all these responses is that they take place as effectively on a palp that has been freshly cut from a clam as on the palp intact. Careful comparative inspection of the reacting palps disclosed no obvious difference between the efficiency of these organs when normally attached to the clam and after they had been severed from it. On cutting a palp from a clam for experimental work, it is well to allow it to remain at least a quarter of an hour in quiet water before subjecting it to stimulation. Such a palp is responsive for about an hour and a half after removal. This condition shows that the palp contains within itself the neuromuscular organization necessary for all the responses described in this paper, and that it, therefore, possesses an autonomy even more complete than that of the vertebrate heart and comparable with what is shown by the tentacle of an actinian.

It is intended to continue the line of investigations suggested by these studies and to extend them to the histology of the parts concerned.

[1] Contributions from the Zoölogical Laboratory of the Museum of Comparative Zoölogy at Harvard College. No. 311.

# THE DEPTH OF THE EFFECTIVE PLANE IN X-RAY CRYSTAL PENETRATION

By F. C. Blake,

Department of Physics, Ohio State University

Communicated by E. H. Hall, May 25, 1918

In determining the value of '*h*' by means of X-rays Blake and Duane[1] (p. 636) found out by experiment that the 'depth of the effective plane' was 0.203 mm. for the case of calcite, using X-rays of a wave-length 0.454 Å. An attempt is made in this note to explain this theoretically.

Call $\mu$ the coefficient of true absorption and $r$ the reflection coefficient. Suppose a parallel beam of X-rays strikes the crystal face at glancing angle $\theta$. Then if $A_0$ is the amplitude of the primary beam the total effect at the ionization chamber is the sum of the various reflections from all those planes of atoms that are able for any reason to play a part at the ionization chamber. Call the last plane of atoms that is thus effective the $m$th plane. Figure 1 will render the situation clear. Let $AB$–$IJ$ represent the parallel beam of X-rays as determined by the slit-width $s$. The reflected beam $bdf$, for instance, is the sum of the various reflections at $b$, $d$, and $f$. Ray $AB$ suffers partial reflection at $A$ and arrives at $a$ with amplitude $A_0 l^{-\mu d \csc \theta}$ where it again suffers reflection. The reflected part has the amplitude $rA_0 l^{-\mu d \csc \theta}$ at $a$ and by the time it gets to $c$ its amplitude has been reduced to $rA_0 l^{-2\mu d \csc \theta}$. Thus the total amplitude along any reflected ray $bdf$ situated a distance $x$ away from the first reflected ray $AI$ is

$$rA_0\left(1 + e^{-2\mu d \csc \theta} + e^{-4\mu d \csc \theta} + \ldots\ldots + e^{-2n\mu d \csc \theta}\right), \text{ in which } n = \frac{x}{2d \cos \theta}.$$

This gives for the amplitude of the ray $bf$,

$$rA_0 \frac{1 - e^{-2\,(n+1)\,\mu d \csc \theta}}{1 - e^{-2\mu d \csc \theta}}, \text{ reducing to } rA_0 \frac{1 - e^{-\frac{\mu x}{\sin \theta \cos \theta}}}{2\mu d \csc \theta},$$

very approximately.

Call $D_1$ the mean depth of the ray $bdf$. Then

$$rA_0 \frac{1 - e^{-\frac{\mu x}{\sin \theta \cos \theta}}}{2\mu d \csc \theta} = n\,r\,A_0 e^{-2\mu D_1 \csc \theta}.$$

Solving for $D_1$ we have

$$D_1 = \frac{\sin \theta}{2\mu} \log_e \frac{\mu x}{\sin \theta \cos \theta \left(1 - e^{-\frac{\mu x}{\sin \theta \cos \theta}}\right)} \tag{1}$$

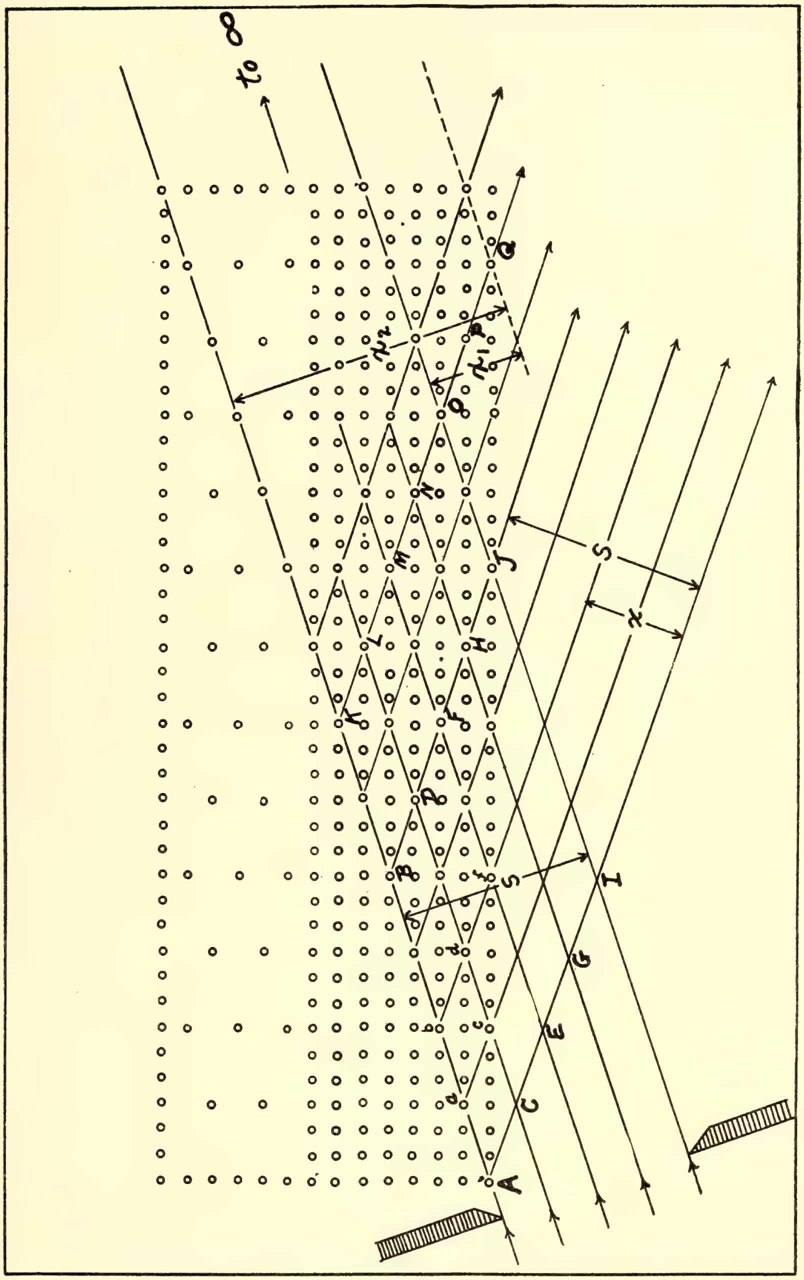
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FIG. 1.

Now the 'effective plane' was defined by Blake and Duane[1] (p. 632) as that plane at which if reflection for all the rays occurred at *that* plane *only* the effect at the ionization chamber would be the same as actually does occur due to the different reflections for all the planes of atoms playing any part. In accordance with this definition, if $D$ is the depth of the 'effective plane' and if for the moment we limit ourselves to that portion of the reflected beam of width $s$, viz., $AI$–$BJ$, it is clear, since for the separate rays we must add not amplitudes but intensities, that we must get $D$ by integrating $D_1$ throughout the region $s$ and taking mean values. Thus if we were concerned only with the reflected beam of width $s$ we could get $D$ from (1) by replacing the parenthesis by

$$\left[\frac{1}{s}\int_0^s \left(1-e^{-\frac{\mu x}{\sin\theta\cos\theta}}\right)^2 dx\right]^{\frac{1}{2}}.$$

But the reflected beam is not of width $s$ only. Rather must we consider the effect of rays that penetrate the crystal to depths much greater than the plane through $B$. Consider for instance the ray $AB$ penetrating to the plane through $K$ say, where it is reflected along the direction $KQ$ and emerges from the crystal at $Q$. At $L$, $M$, $N$, $O$ the ray $KQ$ is reinforced by reflections in phase with one another, but at $P$ and $Q$ there are no reinforcements since the initial beam is determined by the slit width $s$. Accordingly the amplitude of the ray $KQ$ upon emergence is

$$rA_0\left(e^{-2n\mu d\csc\theta}+e^{-2\ n-1)\ \mu d\csc\theta}+\ .\ .\ .\ .\ .+e^{-2q\mu d\csc\theta}\right),$$

where $q$ is the number of the plane through $O$. In other words the amplitude of the ray $KQ$ upon emergence is

$$e^{-\frac{\mu x_1}{\sin\theta\cos\theta}}-e^{-\frac{\mu x_2}{\sin\theta\cos\theta}},$$

where $x_1$ is the distance (measured parallel to the slit $s$) between the incident ray through $Q$ and that through $O$, and $x_2$ is the distance between the ray through $Q$ and that through $K$. Necessarily $x_2-x_1$ must equal $s$. Accordingly to get our mean penetration $D$ for all the rays that penetrate into the crystal beyond the reflected ray $BJ$ we must replace the parenthesis in (1) by

$$\left[\frac{1}{x_2-x_1}\int_s^\infty\left(e^{-\frac{\mu x_1}{\sin\theta\cos\theta}}-e^{-\frac{\mu x_2}{\sin\theta\cos\theta}}\right)^2 dx\right]^{\frac{1}{2}}.$$

Having found the mean penetration for all the rays between 0 and $s$ and that for all the rays between $s$ and $\infty$ it is a simple matter to get the mean of these means, which should be, finally, the depth of the 'effective plane.'

Now in the above expressions the value of $\mu$ is the amplitude coefficient of absorption while what is experimentally measured is the intensity coefficient of absorption. If we accordingly replace $2\mu$ in the above expression by $\mu$ we have as our expressions for the depth of the effective plane the following:

For the X-rays contained in the triangle $ABJ$,

$$D' = \frac{\sin\theta}{\mu}\log_e \frac{\mu s}{2\sin\theta\cos\theta\left[\frac{1}{s}\int_0^s\left(1-e^{-\frac{\mu x}{2\sin\theta\cos\theta}}\right)^2 dx\right]^{\frac{1}{2}}}; \tag{2}$$

and for the X-rays contained in the parallelogram $BK\infty\ \infty OJB$,

$$\begin{aligned} D'' &= \frac{\sin\theta}{\mu}\log_e \frac{\mu s}{2\sin\theta\cos\theta\left[\frac{1}{s}\int_s^\infty\left(e^{-\frac{\mu x}{2\sin\theta\cos\theta}} - e^{-\frac{\mu(x+s)}{2\sin\theta\cos\theta}}\right)^2 dx\right]^{\frac{1}{2}}} \\ &= \frac{\sin\theta}{\mu}\log_e \frac{\mu s}{2\sin\theta\cos\theta\left(1-e^{-\frac{\mu s}{2\sin\theta\cos\theta}}\right)\left[\frac{1}{s}\int_s^\infty e^{-\frac{\mu x}{\sin\theta\cos\theta}}\cdot dx\right]^{\frac{1}{2}}}. \end{aligned} \tag{3}$$

Now Blake and Duane, in determining the maximum positions of the ionization chamber corresponding to a given value of $\theta$ had in one case made $\theta$ equal to 4°18′, which corresponded to a wave-length $\lambda$ equal to 0.454 Å, Using Duane's formula for the absorption, viz., $\mu/\rho_{\mathrm{Al}} = 14.9\ \lambda^3$ we get $\mu/\rho_{\mathrm{Al}} = 1.394$. Applying Bragg's formula for the atomic absorption coefficient, $a$, say, we have

$$a \equiv \mu\omega/\xi = kN^4,$$

where $k$ is a constant and $\omega$ the atomic weight. Thus we get

$$a_{\mathrm{Al}} = 37.79\ a^{-1},\ A_{\mathrm{Ca}} = 211.7\ A^{-1},\ a_{\mathrm{C}} = 1.71\ A^{-1},\ a_{\mathrm{O}} = 16.26\ A^{-1},$$

where $A$ is the number of molecules in a gram molecule, viz. $6.062 \times 10^{23}$. If now we assume the molecular absorption, $m$ say, to be equal to the sum of the atomic absorptions, we have

$$m_{\mathrm{CaCO_3}} = a_{\mathrm{Ca}} + a_{\mathrm{C}} + 3a_{\mathrm{O}} = 229.67\ A^{-1},$$

whence $\mu : \rho_{\ \mathrm{CaCO_3}} = 229.67 : 100.07 = 2.295$; and if we take $\rho$ for calcite to be 2.712, $\mu$ comes but equal to 6.22.

Now W. L. Bragg has shown[2] that for calcite there is only half a molecule to each elementary cell. In other words, the value of $\mu$ we require is $6.22 : \sqrt[3]{2} = 4.94$.

Performing the integrations indicated in (2) and (3) we get

$$D' = \frac{\sin\theta}{\mu}\log_e \frac{\mu s}{2\sin\theta\cos\theta\left[\frac{1}{\mu s}\left\{\mu s + \sin\theta\cos\theta\left(4e^{-\frac{\mu s}{2\sin\theta\cos\theta}} - e^{-\frac{\mu x}{\sin\theta\cos\theta}} - 3\right)\right\}\right]^{\frac{1}{2}}} \tag{4}$$

and

$$D'' = \frac{\sin\theta}{\mu}\log_e\left[\left(\frac{\mu s}{\sin\theta\cos\theta}\right)^{3/2}\frac{1}{\left(1-e^{-\frac{\mu s}{2\sin\theta\cos\theta}}\right)e^{-\frac{\mu s}{2\sin\theta\cos\theta}}}\right].$$

Now in the work of Blake and Duane $s$ was 0.040 cm. Taking $\mu = 4.94$ and $\theta = 4°18'$, $D'$ comes out 0.150 mm.; and $D''$, 0.363 mm.

If we plot the relative intensity, viz.

$$\left(1 - e^{-\frac{-\mu x}{2\sin\theta\cos\theta}}\right)^2,$$

against $x$, we get Curve I of figure 2 for values of $x$ less than 0.04 cm. For values of $x$ greater than 0.04 cm. the relative intensity is

$$\left(e^{-\frac{\mu x}{2\sin\theta\cos\theta}} - e^{-\frac{\mu(x+s)}{\sin\theta\cos\theta}}\right)^2,$$

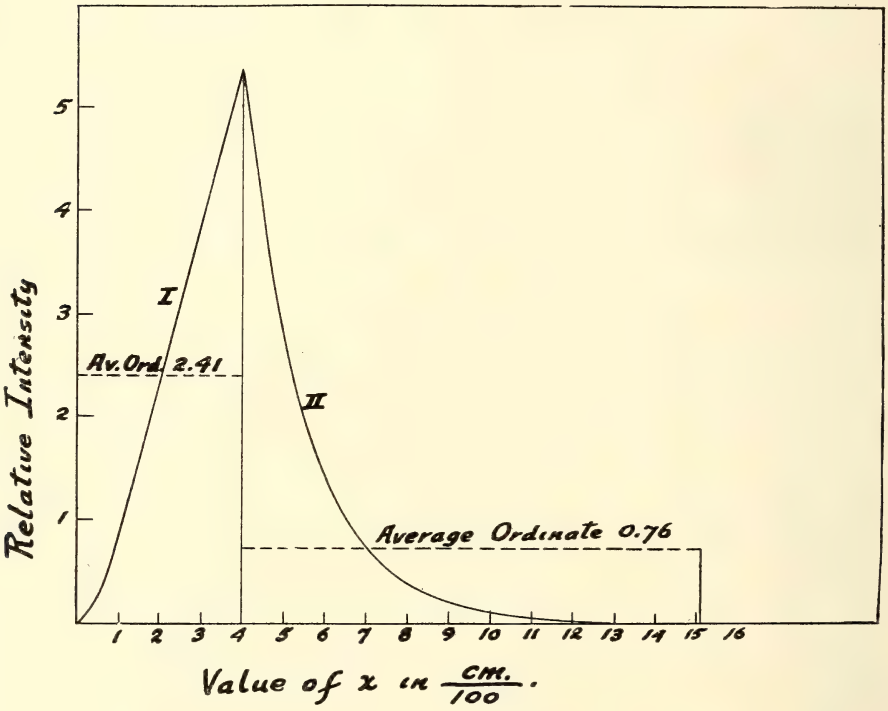

FIG. 2.

and plotting this against $x$, we get Curve II of figure 2. Measured carefully with a planimeter the average ordinate for I is 2.41 and for II it is 0.76. Finally therefore we have as the depth of the effective plane

$$D = \frac{2.41\,D' + 0.76\,D''}{3.17} = 0.201 \text{ mm.}$$

Blake and Duane found $D$ experimentally to be 0.203 mm. Thus the agreement is all that can be expected.

In figure 1, for simplicity sake the writer has assumed the space lattice for calcite to be cubical; as a matter of fact it is rhombohedral. Blake and Duane pointed out[3] that, when crystal penetration is properly corrected for, the curve of atomic number versus square root of frequency is *almost* a straight line, their own values falling more nearly on a straight line than either those of Moseley or de Broglie. As a matter of fact it is manifest that the effective reflected ray for a crystal like calcite is along such a direction as to make the measured angle of reflection proportionately too large for the higher frequencies. In other words as the atomic number increases the measured frequency is proportionately greater than it should be. This possibly accounts for the fact that the curve in figure 3 of Blake and Duane's paper is convex downward toward the axis of atomic numbers. It would seem that a test of this reasoning could be made by repeating the work of Blake and Duane using a crystal with a cubic lattice but one whose faces are as good as those of calcite (rock salt is notoriously bad), care being used of course to correct for crystal penetration, or to eliminate the effect of such penetration by using a very narrow incident beam and having the ionization chamber slit very wide (the arrangement shown in figure 1, first used by Duane and Hunt[4]).

[1] *Physic. Rev. Ithaca*, **10**, 1917, (624–637).
[2] *Proc. Roy. Soc.*, (A), **89**, 1914, (468).
[3] *Physic. Rev. Ithaca*, **10**, 1917, (703).
[4] *Ibid.*, August, 1915, p. 166.

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# *THE MYODOME AND TRIGEMINO-FACIALIS CHAMBER OF FISHES AND THE CORRESPONDING CAVITIES IN HIGHER VERTEBRATES*

By Edward Phelps Allis, Jr.

Palais Carnolés, Menton, France

Communicated by E. L. Mark, June 20, 1918

A functional myodome is found only in fishes, and even among them it is limited, in the specimens I have examined, to Amia and the non-siluroid Teleostei.

The myodome is always separated from the cavum cerebrale cranii by either membrane (dura mater), cartilage or bone, and the separating wall is in part spinal and in part prespinal in position. A depression in the prespinal portion lodges the hypophysis, or both the hypophysis and saccus vasculosus, and this part of the wall never undergoes either chondrification or ossification, a more or less developed pituitary sac always projecting into the myodome.

The myodome is found in its most complete form in the Teleostei, and there consists of dorsal and ventral compartments, which are usually separated from each other by membrane only, but that membrane, the horizontal myodomic membrane, is capable of either chondrification or ossification. The dorsal compartment lodges the hind ends of the musculi recti externi, and is always traversed by a cross-commissural venous vessel formed by the pituitary veins. The ventral compartment lodges the hind ends of the musculi recti interni, and is traversed by the internal carotid and efferent pseudobranchial arteries and the palatine branches of the nervi faciales.

The parasphenoid forms the floor of the ventral compartment of the myodome, and whenever the horizontal myodomic membrane undergoes ossification, the bone so formed forms part of the parasphenoid. This bone is thus certainly, in certain fishes, in part of axial origin, and not simply a dermal bone that has gradually sunk inward to its actual position.

In the Siluridae (Ameiurus) there is apparently a much reduced, but non-functional, dorsal myodomic compartment, but no ventral compartment, that portion of the parasphenoid that lies in the proötic region being developed in what corresponds to the horizontal myodomic membrane of others of the Teleostei.

In Amia the myodome corresponds to the dorsal compartment only of the teleostean myodome, and a strictly similar, but non-functional, myodomic cavity is found in Lepidosteus and Polypterus. The ventral compartment of the teleostean myodome is represented, in each of these three fishes, by a canal, on either side of the head, that is traversed by the internal carotid artery, and that corresponds to the canalis parabasalis of Gaupp's[1] descriptions of higher vertebrates.

The myodomic cavity is limited, in the Holostei and Crossopterygii, to the proötic region, and is there in part subspinal and in part prespinal and subpituitary in position. In the non-siluroid Teleostei examined, the dorsal compartment of the myodome is always more or less prolonged posteriorly into the basioccipital region, and the ventral compartment is frequently so prolonged.

The posterior part of the basioccipital portion of the myodome lies between ventro-lateral vertebral processes that are quite certainly the homologues of the haemal arches of the tail. In Hyodon, this part of the myodome is an open groove and lodges the anterior portion of the median dorsal aorta. In the Cyprinidae, part of this groove has become enclosed to form a short canal, which is traversed by the median dorsal aorta, the enclosing bone forming the pharyngeal process.

The conditions in these fishes thus leads inevitably to the assumption that the entire dorsal myodomic cavity is a subvertebral canal similar to the haemal canal in the tail, and that the dorsal aorta has been excluded from it because of the formation of a circulus cephalicus. What the primary relations of the hypophysis and pituitary veins to this preëxisting canal were, is problematical, but they became lodged in its anterior portion and so gave rise to the conditions actually found in Lepidosteus and Polypterus. The musculi recti externi then secondarily invaded this space by traversing the foramina for the pituitary veins, the other recti muscles retaining their insertions on the external surface of the preclinoid wall, and so gave rise to the conditions found in Amia. The conditions found in the non-siluroid Teleostei then arose as a result of the resorption of the cartilage that, in Amia, forms the preclinoid wall, the pedicel of the alisphenoid, and those ventral portions of the basicapsular commissures that form the lateral walls of the subpituitary portion of the myodome. Because of the resorption of the preclinoid wall, and its replacement by membrane, the musculi recti interni, which in Amia have their points of insertion on either lateral edge of that wall, have first sought firmer attachment on the dorsal surface of the parasphenoid, and have then later pushed posteriorly in the open ends of the persisting portions of the canales parabasales. The fusion of these two canals with each other then formed a ventral myodomic compartment, which is, in early embryos, separated from the dorsal and primary compartment by membrane only; but this membrane may undergo either partial chondrification (Hyodon), or ossification (Gasterosteus) the bone, in the latter case, forming a transverse and inclined ridge on the dorsal surface of the parasphenoid. The membranes resulting from the resorption of the preclinoid wall were then pressed together in the median line by the recti interni, and form a median vertical myodomic membrane, which encloses the internal carotid arteries in a membranous canal that is the homologue of the cartilaginous canals of Amia. The efferent pseudobranchial arteries, pressed downward by the recti interni, lost their connections with the internal carotids and acquired a cross-commissural connection with each other. The membrane resulting from the resorption of the anterior portions of the basicapsular commissures of either side ossified as part of the ascending process of the parasphenoid, and the tissues resulting from the resorption of the pedicel of the alisphenoid ossified, in certain fishes (Cottus, Gasterosteus), to form an anterior portion of that same process.

In the Selachii the myodomic cavities of the Holostei and Teleostei are represented either by canals in the basis cranii that are traversed by the pituitary veins and the internal carotid and efferent pseudobranchial arteries, or by a posterior and deeper portion of the large pituitary fossa of the chondrocranium that is shut off from the cavum cerebrale cranii by the dura mater, and is traversed by the pituitary veins and the internal carotid arteries.

In embryos of Ceratodus there is a subpituitary space, traversed by the pituitary veins, that corresponds to the dorsal compartment of the teleostean-myodome, and the internal carotid canals of Amia have been added to it. This fusion of these canals with the dorsal myodomic cavity is due either to the resorption of the cartilage that separates them in Amia, or to a shifting posteriorly of both the hypophysis and the internal carotides from a position between the hind ends of the trabeculae to one between the so-called anterior prolongations of the parachordals.

In the Amphibia the basis cranii apparently corresponds to the roof, and not the floor, of the dorsal myodomic cavity of Amia and the Teleostei. The fenestra hypophyseos of these animals is, then, the homologue of the pituitary opening of the brain case of fishes.

In the Reptilia and Mammalia a dorsal myodomic cavity is found that is similar to that in Ceratodus. In man it is represented in the cavernous and intercavernous sinuses, and the venous vessels that traverse those sinuses are the homologues of the pituitary veins of fishes.

The cartilago acrochordalis of Sonies's[2] and Noordenbos's[3] descriptions of birds and mammals, respectively, is the homologue of the cartilaginous prootic bridge of embryos of fishes. The open space between this cartilage, or bridge, and the anterior end of the parachordal plate is the fenestra basicranialis posterior properly so called. This fenestra is a perforation of the roof of the myodomic cavity, and hence is not the homologue of the so-called fenestra basicranialis posterior of embryos of fishes, which fenestra is a perforation of the floor of that cavity. This latter fenestra of embryos of fishes is the homologue of the fenestra hypophyseos of birds and mammals, the so-called anterior prolongations of the parachordals of fishes being the homologues of the polar cartilages of birds and mammals.

In certain of the Selachii there is an acustico-trigemino-facialis recess, and there may be certain canals in the cranial wall that are traversed by the vena jugularis and the external carotid artery.

In Amia the trigemino-facialis portion of this recess has fused with the canals for the vena jugularis and the external carotid artery to form a trigemino-facialis chamber, this chamber has become continuous ventrally with the myodome, and the large chamber so formed has been prolonged anteriorly by a space enclosed between the pedicel of the alisphenoid and the primitive side wall of the neurocranium. The foramina for the pituitary vein and the nervi oculomotorius and trochlearis open into this anterior prolongation of the chamber, and, through its orbital opening, into the orbit. The vena jugularis traverses this orbital opening to enter and traverse the trigemino-facialis chamber; the musculus rectus externus traverses it to enter the myodome; and the nervus profundus traverses it to join the ganglion or root of the nervus trigeminus. The nervus trigeminus and the external carotid artery issue from the trigemino-facialis chamber posterior to the pedicel of the alisphenoid and run forward lateral to it.

In the non-siluroid Teleostei the trigemino-facialis chamber is not continuous with the myodome, and it has been separated by a wall of bone into partes ganglionaris and jugularis that correspond, respectively, to the trigemino-facialis recess and the jugular and external carotid canals of the Selachii. The pedicel of the alisphenoid is incomplete, or wholly wanting, but it may be replaced by an anterior prolongation of the ascending process of the parasphenoid. In the latter case the nerves, arteries, veins and muscles all have the same relations to this process that they have to the pedicel of the alisphenoid of Amia. The lateral wall of the pars jugularis of the trigemino-facialis chamber is always less extensive than in Amia, and may be wholly wanting.

In Ceratodus there is a trigemino-facialis chamber similar to that in Amia, and there is a bar of cartilage that corresponds to the pedicel of the alisphenoid of that fish.

In the Amphibia there is a trigemino-facialis recess, and the pars ascendens of the quadrate forms the lateral wall of a space that corresponds to the pars jugularis of the chamber of the Teleostei. The ascending process of the palatoquadrate is the homologue of the pedicel of the alisphenoid of fishes.

In the Reptilia there apparently is no trigemino-facialis recess, the lateral wall of the neurocranium being the primitive cranial wall. The pars ascendens of the quadrate forms the lateral wall of a trigemino-facialis chamber. The antipterygoid (columella) is the homologue of the pedicel of the alisphenoid of fishes, and the processus basipterygoideus the homologue of the floor of the orbital opening of the myodome of Amia.

In the Mammalia there is a trigemino-facialis recess formed by the cava epiptericum and supracochleare. The ala temporalis is peculiar to mammals, and is a bar of cartilage formed between the nervi maxillaris and mandibularis trigemini as they issue from the trigemino-facialis recess, the processus alaris corresponding to some part of the side wall of the prespinal portion of the myodome of Amia. The ala temporalis has been prolonged anteriorly so as to enclose a space that lies anterior to the trigemino-facialis recess, and the foras mina for the pituitary vein (sinus cavernous) and the nervi oculomotorius, trochlearis and profundus (first branch of trigeminus) open into this apace and from it into the orbit. The cavum tympanicum is the pars jugularis of the trigemino-facialis chamber, and the processus pterygoideus, the malleus, incus and stapes, and possibly also the annulus tympanicus, are quite certainly portions of the lateral wall of that part of the chamber. A diverticulum of the spiracular canal, or an independent evagination of the pharynx, has expanded into this part of the chamber and so formed the middle ear. The chorda tympani must, then, correspond to that communicating branch from the nervus facialis to the nervus trigeminus that, in fishes, traverses the trigemino-facialis chamber, and hence must be a prespiracular nerve.

The internal carotid artery enters the cranial cavity, in most vertebrates, by passing upward mesial to the related trabecula, or mesial to that posterior prolongation of the trabecula that is formed by the polar cartilage, but in Ameiurus it enters the cranial cavity through the foramen opticum, and hence would seem there to pass lateral and then dorsal to the trabecula. In embryos of the Mammalia ditremata also this artery is said to pass upward lateral to the trabecula, but it is probable that it here simply passes lateral to the infrapolar process of the polar cartilage, the latter cartilage itself not fusing directly with the parachordal plate, its direct relations to the artery thus being obscured.

[1] Gaupp, E., *Anat. Anz., Jena,* **27,** 1905, (273–310).

[2] Sonies, F., *Petrus Camper,* Deel. 4, Jena, 1907, (395–486), Taf. 7/10.

[3] Noordenbos, W., *Ibid.,* Deel. 3, Jena, 1905, (367–430), Taf. 6/8.

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# *THE EFFECT OF INBREEDING AND CROSSBREEDING UPON DEVELOPMENT*[1]

By D. F. Jones

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Communicated by T. B. Osborne, July 12, 1918

The experiments dealing with the effects of inbreeding and crossbreeding in the naturally cross-pollinated corn plant, *Zea mays* L., carried on at the Connecticut Experiment Station have been reported on previously by East[2] and Hayes.[3] A continuation of these experiments shows that there is no appreciable reduction in vigor and productiveness, as indicated by the yield of grain and height of plant, after the eighth generation of self-fertilization as can be seen in table 1. The resulting inbred strains, now in the eleventh

TABLE 1

The Effect of Inbreeding on the Yield and Height of Maize

| YEAR GROWN | NUMBER OF GENERATIONS SELFED | FOUR INBRED STRAINS DERIVED FROM A VARIETY OF LEAMING DENT CORN | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | 1-6-1-3-etc. | | 1-7-1-1-etc. | | 1-7-1-2-etc. | | 1-9-1-2-etc. | |
| | | Yield per acre | Height | Yield per acre | Height | Yield per acre | Height | Yield per acre | Height |
| | | *bushels* | *inches* | *bushels* | *inches* | *bushels* | *inches* | *bushels* | *inches* |
| 1916 | 0 | 74.7 | 117.3 | 74.7 | 117.3 | 74.7 | 117.3 | 74.7 | 117.3 |
| 1905 | 0 | 88.0 | | 88.0 | | 88.0 | | 88.0 | |
| 1906 | 1 | 59.1 | | 60.9 | | 60.9 | | 42.3 | |
| 1908 | 2 | 95.2 | | (1907) 59.3 | | (1907) 59.3 | | 51.7 | |
| 1909 | 3 | 57.9 | | (1908) 46.0 | | (1908) 59.7 | | 35.4 | |
| 1910 | 4 | 80.0 | | 63.2 | | 68.1 | | 47.7 | |
| 1911 | 5 | 27.7 | 86.7 | 25.4 | 81.1 | 41.3 | 90.5 | 26.0 | 76.5 |
| 1912 | 6 | | | | | | | (1913) 38.9 | |
| 1913 | 7 | 41.8 | | 39.4 | | | | (1914) 45.4 | 85.0 |
| 1914 | 8 | 78.8 | 96.0 | 47.2 | 83.5 | 58.5 | 88.0 | (1915) 21.6 | |
| 1915 | 9 | 25.5 | | 24.8 | | | | (1916) 30.6 | 78.7 |
| 1916 | 10 | 32.8 | 97.7 | 32.7 | 84.9 | 19.2 | 86.9 | (1917) 31.8 | 82.4 |
| 1917 | 11 | 46.2 | 103.7 | 42.3 | 78.6 | 37.6 | 83.8 | | |

generation of continuous self-fertilization, are remarkably uniform and constant. Very little change in average ear row number or reduction in variability of row number has taken place after the eighth generation in two inbred lines derived originally from the same plant in the second generation of inbreeding as shown in figure 1. These results are in agreement with the theoretical attainment of nearly complete homozygosis automatically in about the eighth generation of self-fertilization. Although the inbred strains are now only about one half as productive as the original variety, the plants, judging from their behavior in the past three years, are capable of being maintained unchanged indefinitely by self-fertilization.

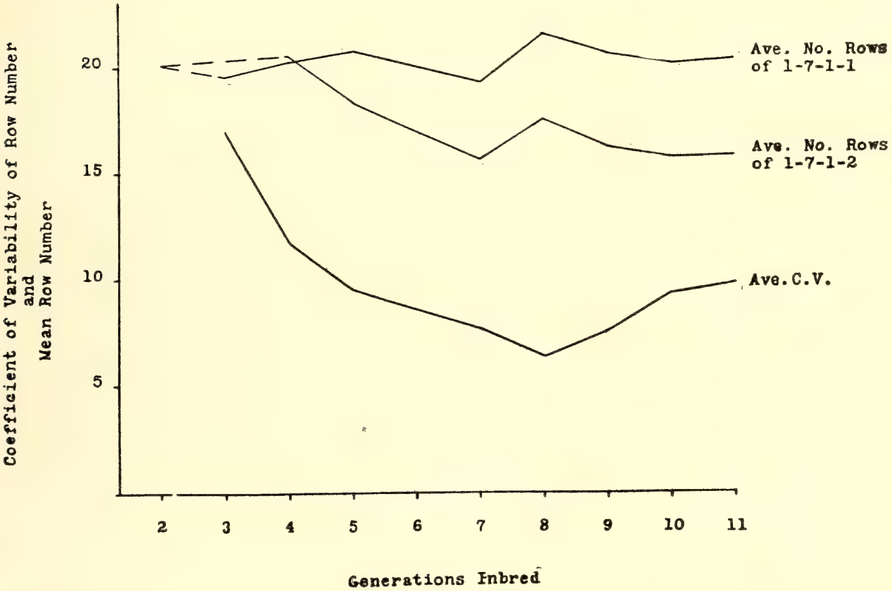

FIG. 1.

In contrast to the uniformity of all the plants within one inbred line is the difference between the several lines. With regard to ear characters two of them have flat cobs and two have round. One has uncolored cobs while the others have colored. They all differ in the shape and size of ears and seeds and in the arrangement of the seeds on the ears. Equally noticeable differences characterize the tassels, stalks and leaves.

One of the most pronounced effects of inbreeding in maize is the appearance of sterility in the form of pollen and ovule abortion. An extreme reduction in the amount of pollen produced is shown frequently in plants long inbred. Not all strains show this, however, as some have well developed anthers and produce abundant pollen. In nearly every case, however, those inbred strains which have the best developed staminate inflorescences have poorly developed pistillate inflorescences and those strains which have the largest pistillate inflorescences and are most productive of grain produce a very small quantity of pollen. Doubtless many plants are produced which completely lack one or the other or both functions but such types, of course, can not be maintained in self-fertilization. There is, therefore, a decided tendency for inbreeding to change a monoecious plant into a functionally dioecious plant.

All of the inbred strains obtained show an immediate return to the vigorous condition of the non-inbred parents when crossed. The return of vigor is greater in some combinations than in others. In general there seems to be a correlation between the productiveness of the female parent strain and the productiveness of the first generation hybrid. This is in part due to the fact that hybrid plants start from better developed seeds when the female parent is the more vigorous. It is also shown in a comparison of the $F_2$ generation, starting from large seeds grown on vigorous hybrid plants, with the $F_1$ generation starting from seeds produced on the reduced inbred plants. The first generation overcomes this handicap and in every case may be expected to surpass the second or subsequent generations as shown by the curves in figure 2.

The manifestation of heterosis is greater in some characters than in others. In the many crosses between inbred strains observed yield of grain is increased 180%, height of plant 27%, length of ear 29%, number of nodes 6% and the number of rows of grain on the ear 5%. Abundant evidence has been obtained from these inbred strains to show that the endosperm also is immediately affected by crossing. Differences in weight of reciprocally crossed seeds as compared to selfed seeds developed in the same inflorescences have averaged from 15 to 20% in favor of the cross. Furthermore in spite of this increase in weight the hybrid seeds mature faster as shown by their lower water content. Due in part to this latter fact there is a noticeable difference in viability as shown by the per cent of germination between the selfed and crossed seeds produced by the same plants. The crossed seedlings also appear from one to two days earlier than the selfed seedlings.

In view of the notable advantages to be gained from cross-fertilization it was thought that when mixtures of a plant's own pollen with that of a distinct sort were applied there might be a selective fertilization favoring the foreign pollen. By taking advantage of xenia it was possible to obtain and to distinguish selfed and reciprocally crossed seeds from plants of two different inbred strains. Since the same mixture of pollen was applied to plants of both strains, the number of seeds produced by one kind of pollen should be in the same ratio to the number of seeds produced by the other kind of pollen, on both types of plants to which the mixture was applied, irrespective of the amounts or viability of the two kinds of pollen used. Any deviation from a perfect proportion in the form of an excess of crossed seeds or an excess of selfed seeds would indicate selective fertilization in one or the other direction. A large amount of data has been obtained in this way which shows that there is no inequality in fertilizing ability favoring foreign pollen. In fact the data show a selection in the other direction large enough to have

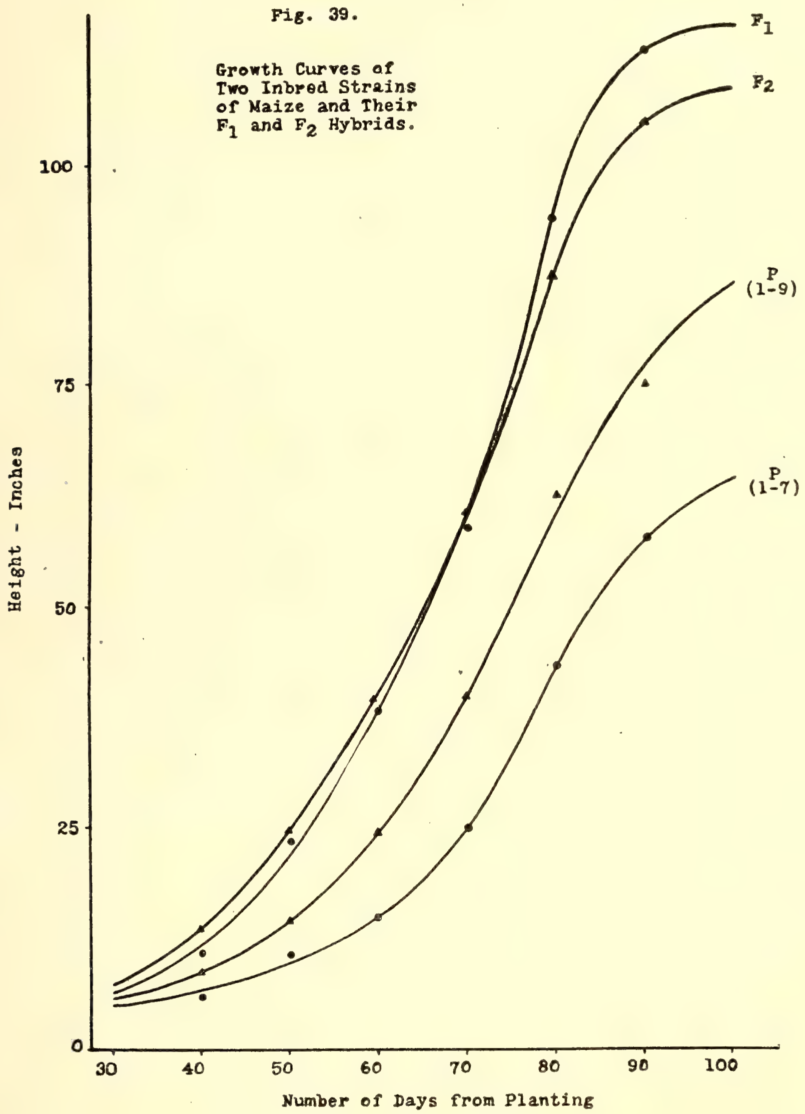

FIG. 2.

significance when compared to the probably error. Whether this is really the case or whether there has been a constant error in classifying the seeds will have to await the growing of the plants from these seeds in order to determine the accuracy of the classification. In the meantime it hardly seems possible that the error could be so great as to hide an actual selective fertilization in favor of cross-pollination, since the inbred strains are so uniform in color of endosperm and the crossed seeds usually so distinct that the error of classification is small and presumably not all in one direction. And if there is no selection in favor of cross-fertilization, there is then no effect of crossing until the zygote is formed however great the advantages immediately accruing to the resulting embryos and endosperms.

[1] An extract from a treatise submitted to the faculty of the Bussey Institution of Harvard University in partial fulfillment of the requirements for the degree of Doctor of Science, December, 1917, and to be published in full as a contribution from the Connecticut Agricultural Experiment Station.

[2] East, E. M., *Connecticut Agric. Exp. Sta. Rep.*, 1907, 1908, (419–428); *Amer. Nat., Lancaster, Pa.*, **43**, 1909, (173–181).

[3] East, E. M., and Hayes, H. K., *Washington, U. S. Dept. Agric., Bur. Plant Ind. Bull.* No. 243, 1912.

# *NATIONAL RESEARCH COUNCIL*

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## EXECUTIVE ORDER ISSUED BY THE PRESIDENT OF THE UNITED STATES MAY 11, 1918

The National Research Council was organized in 1916 at the request of the President by the National Academy of Sciences, under its Congressional charter, as a measure of national preparedness. The work accomplished by the Council in organizing research and in securing co-operation of military and civilian agencies in the solution of military problems demonstrates its capacity for larger service. The National Academy of Sciences is therefore requested to perpetuate the National Research Council, the duties of which shall be as follows:

1. In general, to stimulate research in the mathematical, physical and biological sciences, and in the application of these sciences to engineering, agriculture, medicine and other useful arts, with the object of increasing knowledge, of strengthening the national defense, and of contributing in other ways to the public welfare.
2. To survey the larger possibilities of science, to formulate comprehensive projects of research, and to develop effective means of utilizing the scientific and technical resources of the country for dealing with these projects.
3. To promote co-operation in research, at home and abroad, in order to secure concentration of effort, minimize duplication, and stimulate progress; but in all co-operative undertakings to give encouragement to individual initiative, as fundamentally important to the advancement of science.
4. To serve as a means of bringing American and foreign investigators into active co-operation with the scientific and technical services of the War and Navy Departments and with those of the civil branches of the Government.
5. To direct the attention of scientific and technical investigators to the present importance of military and industrial problems in connection with the war, and to aid in the solution of these problems by organizing specific researches.
6. To gather and collate scientific and technical information at home and abroad, in co-operation with Governmental and other agencies and to render such information available to duly accredited persons.

Effective prosecution of the Council's work requires the cordial collaboration of the scientific and technical branches of the Government, both military and civil. To this end representatives of the Government, upon the nomination of the National Academy of Sciences, will be designated by the President as members of the Council, as heretofore, and the heads of the departments immediately concerned will continue to co-operate in every way that may be required.

(Signed) WOODROW WILSON.

THE WHITE HOUSE
*May 11, 1918.*

# *NATIONAL RESEARCH COUNCIL*

## MINUTES OF THE SECOND MEETING OF THE EXECUTIVE BOARD OF THE WAR ORGANIZATION IN JOINT SESSION WITH THE COUNCIL OF THE NATIONAL ACADEMY OF SCIENCES

Tuesday, March 26, 1918

The meeting convened at 9.30 a.m. in the offices of the National Research Council, 1023 Sixteenth Street, Washington, D. C., with President Walcott of the National Academy of Sciences, in the chair.

Present: Members of Council of National Academy of Sciences: Whitman Cross, George E. Hale, Arthur A. Noyes, Charles D. Walcott; members of Executive Board of National Research Council: Whitman Cross, Gano Dunn, George E. Hale, John Johnston, Vernon L. Kellogg, Charles E. Mendenhall, Robert A. Millikan, Arthur A. Noyes, Richard M. Pearce, S. W. Stratton, Charles D. Walcott, and Robert S. Woodward, and by invitation: Henry M. Howe and S. L. G. Knox.

The minutes of the first meeting of the Executive Board of the War Organization of the Research Council, of February 26, 1918, were presented and approved.

The Chairman of the Research Council reported:

1. That the Rockefeller Foundation has made an appropriation of $50,000 to provide for the expenses of the Division of Medicine and Related Sciences for the period from March 1 to December 31, 1918, and that sums, as needed, from this appropriation, will be deposited with the National Academy of Sciences for disbursement upon approval of the Chairman of this Division.

2. That Dr. W. W. Keen has donated the sum of $382.22 for the work of the Council, and that this amount, representing proceeds from sales of a book entitled *The Treatment of War Wounds*, which was prepared by him in connection with his activities as a member of the Council, has been deposited to the credit of the Council with the National Academy of Sciences.

3. That Graham Edgar has assumed his duties in Washington as Technical Assistant of the Research Information Committee, and that W. M. Gilbert will act as Secretary of the Washington office of this Committee.

4. That the Engineering Foundation has approved the designation of the Secretary of the Foundation as Assistant Secretary of the National Research Council, as authorized by the Executive Board of the Council at its meeting of February 26, 1918; and that the Engineering Foundation has reciprocally appointed the Secretary of the National Research Council as an Assistant Secretary of the Engineering Foundation.

The Chairman of the Council submitted the following report of action taken by the Interim Committee of the Executive Board of the Council at a meeting held on Tuesday, March 19:

1. That by reason of his appointment as Chief of the Chemical Service Section of the National Army, Mr. Bogert has resigned as Chairman of the Division of Chemistry and Chemical Technology of the Council, and that upon nomination of the Executive Committee of the Division, Mr. John Johnston has been elected Chairman.

2. That the resignations of Mr. E. G. Conklin as Chairman of the Zoölogy Committee and of Mr. W. H. Holmes as Chairman of the Anthropology Committee of the Council, have been accepted.

3. That an Executive Committee of the Division of Agriculture, Botany, Forestry, Fisheries and Zoölogy, has been appointed as follows: Vernon L. Kellogg, Chairman, Irving W. Bailey, Karl F. Kellerman, Burton E. Livingston, H. F. Moore, Raphael Zon.

4. That a Committee on Medical Zoölogy of the Division of Medicine and Related Sciences has been appointed as follows:

Representatives of Entomology:

Leland O. Howard, U. S. Department of Agriculture, Washington, D. C. (Chairman of Group).

Charles T. Brues, Bussey Institute, Forest Hills, Massachusetts.

W. A. Riley, Cornell University, Ithaca, New York.

Representatives of Protozoölogy:

C. A. Kofoid, University of California, Berkeley, California (Chairman of Group), (Major S. C. N. A., Fort Sam Houston, Texas).

Theobald Smith, Rockefeller Institute, Princeton, New Jersey.

Frederick G. Novy, University of Michigan, Ann Arbor, Michigan.

Representatives of Helminthology:

Henry B. Ward, University of Illinois, Urbana, Illinois (Chairman of Group).

Charles W. Stiles, U. S. Public Health Service, Washington, D. C.

Allen J. Smith, University of Pennsylvania, Philadelphia, Pennsylvania.

5. That a Sub-Committee of the Committee on Psychology, to deal with pedagogical and psychological problems of Military Training and Discipline, has been appointed as follows, upon recommendation of the Chairman of the Division of Medicine and Related Sciences:

William C. Bagley, Carnegie Foundation for the Advancement of Teaching, Chairman.

Charles H. Judd, University of Chicago.

A. Caswell Ellis, University of Texas.

6. That upon recommendation of the Vice-Chairman of the Division of Geology and Geography, and in accordance with action of the Executive Board at its meeting of February 26, an allotment of $1,000 or so much thereof as may be necessary, has been authorized from funds available to the National Research Council, in case such sum cannot be obtained from the U. S. Government, to provide for the expenses of an assistant to Major D. W. Johnson in Europe, when engaged in work for the Division of Geology and Geography, the disbursement of this allotment to be made through the Research Information Committee in Paris. Major Johnson, while assigned primarily to other duty in Europe, has obtained permission and has been formally authorized to act as the representative of the Division of Geology and Geography.

The Chairman of the Council furthermore reported the following recommendations:

1. From the Executive Committee of the Division of Chemistry and Chemical Technology:

(a) That the following be added to membership in the Chemistry Committee:

William J. Comstock, formerly of Chemistry Department, Yale University.

Samuel Avery, Chancellor, University of Nebraska.

(b) That the special Committee on Platinum and Related Metals of the Chemistry Committee, be changed to a Sub-Committee, and that a Sub-Committee on Primers of the Chemistry Committee be appointed as follows:

Richard C. Tolman, Chairman, C. McPhail Smith, H. E. Eastlack.

(c) That nominations submitted by the Institute of Chemical Engineers for membership in the Sub-Committee on Chemical Engineering of the Chemistry Committee, be accepted as follows:

Charles F. McKenna, New York.
G. W. Thompson, Chief Chemist, National Lead Company.
Richard K. Meade, Consulting Chemical Engineer, Brooklyn.
Hugh K. Moore, Chief Chemist and Chemical Engineer, Berlin Mills Company.
William C. Carnell, care of Chas. Lennig & Co., Philadelphia.

2. From the Division of Physics, Mathematics, Astronomy and Geophysics:

That the following be added to membership in the Sub-Committee on Location of Invisible Aircraft of the Physics Committee:

Henry N. Russell, E. O. Dietrick, O. Stuhlman.

Upon motion, the acts of the Interim Committee of the Council as reported above were ratified and confirmed, and the recommendations submitted from the Division of Chemistry and Chemical Technology and from the Division of Phycics, Mathematics, Astronomy and Geophysics, were approved and the appointments made.

A vote of thanks to Mr. Bogert was offered and adopted, in appreciation of his successful labors and effective coöperation as Chairman of the Division of Chemistry and Chemical Technology. The resignations of Mr. Conklin and of Mr. Holmes were also accepted with expression of appreciation of the valuable work they accomplished.

Votes of thanks and appreciation were adopted relative to the donations for the work of the Council, received from the Rockefeller Foundation and from Dr. W. W. Keen.

Upon nomination of the Chairman, Mr. Noyes was elected Chairman of the Administrative Division of the Council.

Upon nomination of the Chairman, the President of the National Academy of Sciences was requested to appoint the following additional member of the Council:

Walter S. Gifford, Director, Council of National Defense.

Upon motion Mr. Gifford was also appointed a member at large of the Executive Board of the Council, and a member of the Military Division.

Mr. French, editor of the *Journal of the American Society of Mechanical Engineers*, appeared before the Board, upon the suggestion of Mr. Dunn, and explained a proposal previously submitted by this Society to supply titles and abstracts of published papers relating to research in engineering and allied subjects, offering to place the facilities of their organization at the disposal of the Council and its Research Information Committee. With a cordial vote of thanks to the American Society of Mechanical Engineers for this evidence of continued assistance to and coöperation with the Research Council, the Executive Board accepted the offer submitted by Mr. French, and accordingly the Research Information Committee was authorized to formulate definite methods for effective coöperation.

Mr. Walcott called attention to the fact that by action of the Executive Committee of the Research Council, in January, 1917, all government appointees of the Council residing in Washington became thereupon members of the Military Committee. Discussion revealed the desirability of limiting the membership of this Committee and of the newly organized Military Division of the Council, and accordingly it was

RESOLVED: That the Military Division of the Council consist of the heads or acting heads of government bureaus in Washington, who have been appointed members of the National Research Council.

Membership of the Division at present consists of:

Charles D. Walcott, Chairman
S. W. Stratton, Vice-Chairman
Carl L. Alsberg
Admiral William S. Benson
Maj.-General William M. Black
Howard E. Coffin
Maj.-General William Crozier
Rear-Admiral Ralph Earle
Walter S. Gifford
Major-General Willliam C. Gorgas
Rear Admiral Robert S. Griffin
Brigadier-General P. D. Lockridge
Van H. Manning
Charles F. Marvin
George O. Smith
Major-General George O. Squier
Rear-Admiral David W. Taylor
Colonel Ralph H. Van Deman
Captain Roger Welles

Mr. Cross reported that the proposed allotment of a sum not to exceed $5,000 as discussed at the meeting of the Executive Board of February 26 and referred to the Interim Committee with authority to act, has become unnecessary in view of a change of plan relative to the special monograph work concerned. On the other hand, he submitted a request from the Military Intelligence Section of the Army for advice and assistance in connection with other monograph work.

After discussion it was voted that the National Research Council accept the invitation of the Chief of the Military Intelligence Section, Executive Division of the General Staff, U. S. A., to furnish a list of the names of persons who can supply information desired for proposed special monographs and that a committee of three be appointed by the Chairman of the Council to take charge of this work. Thereupon, Mr. Hale appointed this committee as follows:

Whitman Cross, Chairman, Henry M. Howe, Alfred D. Flinn.

Upon motion, a special committee of three members of the National Research Council, consisting of Messrs. Dunn, Millikan and Noyes, was appointed to serve in joint session with the special committee of the National Academy of Sciences, referred to above, for the consideration and formulation of proposals relative to the future extension of the work of the Academy and Council, including the accumulation of endowment funds and the construction of suitable buildings.

Mr. Hale reported that Dr. Arthur Schuster, Secretary of the Royal Society, had suggested the desirability of organizing an international conference of scientific academies during the coming summer. Upon motion, the Chairman of the meeting was requested to appoint a committee of three members of the National Academy of Sciences to consider this question. Mr. Hale, Mr. Noyes and Mr. Howe were appointed members of such a Committee, the President of the National Academy of Sciences to act also as an ex officio member.

The meeting adjourned at 12.10 p.m.

## MINUTES OF THIRD MEETING OF EXECUTIVE BOARD OF WAR ORGANIZATION

Tuesday, April 30, 1918

The meeting convened at 9.30 a.m. in the offices of the Council at 1023 Sixteenth Street, with Mr. Hale, Chairman of the Council, in the chair.

Present: Whitman Cross, George E. Hale, Henry M. Howe, John Johnston, Vernon Kellogg, Van H. Manning, John C. Merriam, Robert A. Millikan, Richard M. Pearce, S. W. Stratton, Charles D. Walcott, William H. Welch, Robert S. Woodward, and Robert M. Yerkes, and by invitation, Graham Edgar.

The minutes of the Second Meeting of the Executive Board of the War Organization, of March 26, 1918, were presented and approved.

The Chairman of the Council reported:

1. That a suggestion for the International Organization of Science and Research has been submitted to and approved by the Council of the National Academy of Sciences and has been forwarded to the President of the United States and to the Secretary of State for ratification, and to representatives of the Academy abroad, for presentation, if ratified, at a preliminary international conference of scientific academies to be held in London on May 8, 1918.

The Chairman of the Council presented the following report of action taken by the Interim Committee of the Executive Board of the Council at meetings held on Thursday, April 18, and Monday, April 29.

1. That the following appointments have been made:

Arthur A. Noyes, Chairman of Interim Committee.

J. C. Merriam, Chairman of Section on Relations with Educational Institutions and State Research Councils.

Henry M. Howe, Chairman of Engineering Division.

L. H. Baekeland, Acting Chairman of the Patent Office Committee.

C. E. McClung, member and acting Chairman of Zoölogy Committee.

Oakes Ames and C. E. McClung, members of Executive Committee of Division of Agriculture, Botany and Zoölogy.

John W. Baird, Professor of Psychology, Clark University, Vice-Chairman of Psychology Committee.

Rufus I. Cole, Director of Rockefeller Hospital, member of Executive Committee of Division of Medicine and Related Sciences.

Reid Hunt, Professor of Pharmacology, Harvard Medical School, Representative of medical profession as member of Patent Office Committee.

E. G. Martin, Professor of Physiology, Leland Stanford University, member of Sub-Committee on Muscular Work and Fatigue of Physiology Committee.

Dwight W. Fisher, Chief Clerk of the Council.

2. That the resignation of Colonel William H. Walker as Vice-Chairman of the Division of Chemistry and Chemical Technology has been accepted.

3. That the following have hitherto accepted appointment as members of the Advisory Committee of the Section on Industrial Research: Elihu Root, Theodore N. Vail, Henry S.

Pritchett, Edwin Wilbur Rice, Jr., George Eastman, A. W. Mellon, Pierre S. duPont, and Elbert H. Gary.

4. That the following special Committee on Fertilizers has been appointed, to be composed of two representatives each from the Division of Chemistry and Chemical Technology and the Division of Agriculture, Botany and Zoölogy, said Committee to report to the Chairman of each of these Divisions:

Samuel Avery, Chairman, Chancellor, University of Nebraska.
Homer J. Wheeler, American Agricultural Chemical Company.
Karl F. Kellerman, U. S. Dept. of Agriculture.
Albert F. Woods, President, Maryland State College.

5. That it is inadvisable at present to organize a special section on Governmental Relations of the Administrative Division.

6. That the Research Information Committee shall be considered as the Information Section of the Administrative Division.

7. That the National Advisory Committee for Aeronautics has been invited to act as the Section on Aeronautics of the Engineering Division and that Dr. Charles D. Walcott has been asked to serve as Chairman of this Section.

8. That the title of the Sub-Committee on Fatigue and Industrial Pursuits of the Physiology Committee has been changed to the Sub-Committee on Muscular Work and Fatigue.

9. That the name of the Section on Design of the Engineering Division has been changed to the Section on Mechanical Engineering.

10. That the personnel and work of the Sub-Committee on Biochemistry of the Division of Chemistry and Chemical Technology has been transferred to the jurisdiction of the Division of Medicine and Related Sciences.

11. That in view of existing requirements for war purposes and of the impossibility at present of carrying out plans as originally outlined by the Anthropology Committee, it was voted to substitute for this Committee a number of special committees to cover the particular needs within this field as they arise. The following special committees have been appointed:

Anthropometry in the Army:
C. B. Davenport, Chairman, Ales Hrdlicka, Secretary, E. A. Hooton.
Race in Relation to Disease:
Military Section: Col. A. G. Love, Chairman, Major F. H. Garrison, W. F. Willcox.
Civilian Section: F. S. Hoffman, Chairman.

12. That the use of franking privileges by committees or subcommittees of the Council will be permitted only upon authorization by the Interim Committee.

13. That it has been decided to issue a pamphlet concerning the War Organization of the Council, to include the names of the General Officers and Executive Board, and the names and Chairmen of the various Divisions and of their Sections and Committees.

14. That a section on electrical engineering of the Engineering Division has been established, and that Comfort A. Adams has been appointed chairman of this section, provided it will be possible for him to take active charge of the work.

15. That upon nomination of the Chairman of the Council, the President of the National Academy of Sciences has been requested to appoint the following additional member of the Council:

Henry S. Graves, Forester and Chief of the Forest Service.

Mr. Graves was also named as member of the Military Division.

16. That the following allotments have been authorized upon recommendation of the Division of Medicine and Related Sciences to be paid from the appropriation made by the Rockefeller Foundation for the expenses of this Division.

(a) A sum of $500.00 or as much thereof as may be necessary to meet the traveling expenses of the members of the Sub-Committee on Shock of the Physiology Committee in connection with a conference of this Sub-Committee to be held in Baltimore on April 19 and 20.

(b) A sum of $1,000.00 to Dr. D. R. Hooker, Johns Hopkins Medical School, for experimental investigations to be carried on at the Sandy Hook Proving Station on the influences of high explosives in producing conditions of shock.

(c) A sum of $100.00 to Dr. William Moore to cover expenses of collecting, breeding and storing lice in connection with his investigations of the louse problem in relation to trench fever.

17. That the following allotments have been authorized for investigations under the auspices of the Division of Agriculture, Botany and Zoölogy to be paid from the appropriation made available by the Carnegie Corporation of New York for the general expenses of the Council:

(a) A sum of $1,000.00 for an investigation of the relation of rodent pests to their environment, especially with relation to forage and grain crops, the work to be carried on in coöperation with the U. S. Biological Survey.

(b) A sum of $600.00 for an investigation of lac and lac insects in the South-West including provision for traveling expenses and maintenance of a field assistant in carrying on a survey of the abundance of this material and of the possibility of its exploitation, and in undertaking at the same time a study of the growth and of the commercial possibilities of mesquite gums.

(c) A sum of $2,000.00 for active coöperation with the War Emergency Board of the American Association of Plant Pathologists for the purpose of aiding the work of this Board.

18. That the following allotment has been authorized for expenditure under the auspices of the Engineering Division, to be charged against the appropriation made available by the Carnegie Corporation of New York for the general expenses of the Council:

(a) A sum of $4,000.00 for traveling and incidental expenses of members of the Division.

After discussion of special recommendations submitted by the Interim Committee, the following resolutions were adopted by the Executive Board:

Resolved, That in view of the demands of the war situation, the general policy of the National Research Council shall be to replace the general committees previously established by special committees for specific needs unless it is possible for such existing committees to serve effectively for war purposes.

Resolved, That in view of the importance, particularly under war conditions, of securing regular attendance at meetings of the Executive Committees of the Divisions of the Council each Division be and hereby is authorized and requested to formulate regulations with regard to such attendance, due consideration being given to the needs and work of the Division in question.

Resolved, That the Divisions of the Council be and hereby are requested if practicable to arrange for regular and stated meetings of the Executive Committees of the Divisions during the first week of each month, and that regular meetings of the Executive Board shall be held on the second Tuesday of each month.

Resolved, That the Treasurer be and hereby is authorized to sign checks drawn on available funds of the National Research Council to cover traveling expenses of members of the Executive Board of the Council, and traveling and incidental expenses in connection with the prosecution of work of the Divisions of the Council; vouchers for such expenditures to be submitted to the Treasurer with the approval of the Chairman of the Division concerned or of the Chairman of the Council, as circumstances require.

The Chairman of the Council also reported that the Italian Ambassador in Washington has received a request from the Italian Minister of Munitions for the establishment of an office of the Committee in Rome to undertake work similar to that performed by the offices in London and Paris. After discussion, the Chairman of the Council was authorized to appoint a representative of the Research Information Committee for the proposed office in Rome, in case the Council of National Defense provides for the establishment of this office.

Mr. Howe, Chairman of the Engineering Division, outlined plans for active coöperation with the national engineering societies primarily with reference to war needs, but also as a preparation for future work. Upon motion, the Executive Board approved his program and authorized him to proceed with its development and application.

Mr. Johnston, Chairman of the Division of Chemistry and Chemical Technology, after outlining briefly the activities of his Division, which include frequent conferences and the organization of small groups of active workers to deal with special problems, was authorized, upon motion, to proceed with his program and complete the plan of organization of the Division.

Mr. Merriam, Chairman of the Division of Geology and Geography, reported that, after due consideration, it has been decided to continue the Committees previously established by the Council in these fields as active sub-committees of the divisional organization, and that additional special committees will be appointed with the understanding that all such units of organization will report to the Chairman of the Division. Colonel Welch enquired whether the Division of Geology and Geography is in a position to furnish recommendations, after investigation, of desirable sites for training camps in the United States. The Chairman of the Division was requested to confer with Colonel Welch on this subject.

Colonel Millikan outlined briefly his proposal for weekly conferences of members of the Physics and Engineering Divisions of the Council, and was authorized, upon motion, to proceed with the organization of such conferences.

Upon nomination by Colonel Millikan, the following Executive Committee of the Division of Physics, Mathematics, Astronomy, and Geophysics, was appointed: Joseph S. Ames, Louis A. Bauer, Arthur L. Day, Charles F. Marvin, Major Charles E. Mendenhall, Lieut. Col. R. A. Millikan, F. R. Moulton, Ernest F. Nichols, H. N. Russell, W. C. Sabine, Frank Schlesinger, General George O. Squier, Samuel W. Stratton, Robert S. Woodward.

Mr. Pearce, Chairman of the Division of Medicine and Related Sciences, reported upon the progress of the work of his Division, specially with regard to satisfactory coöperation which is being maintained with the Surgeon General's Office of the Army.

Mr. Kellogg, Chairman of the Division of Agriculture, Botany and Zoölogy, outlined briefly the nature of problems which are under consideration by his Division in relation to food production and conservation, and reported especially upon work of coördination with the United States Department of Agriculture.

Discussion took place relative to the appointment as members of the National Research Council, of all members of the Executive Committees of the Divisions of the Council, and after due consideration it was

RESOLVED, That future appointments to membership in the National Research Council shall be limited to the period of the war, and that the terms of office of all members of the Council, as such, shall expire at the end of the war, pending the establishment of a permanent organization for the Council.

Upon motion, the President of the National Academy of Sciences was requested to appoint the following additional members of the Council, who, at present, are members of Executive Committees of Divisions:

Oakes Ames, Assistant Professor of Botany, Harvard University.

Irving W. Bailey, Assistant Professor of Forestry, Harvard University.

Louis A. Bauer, Director, Dept. of Terrestrial Magnetism, Carnegie Institution of Washington.

Karl F. Kellerman, Associate Chief, Bureau of Plant Industry, U. S. Dept. of Agriculture.

Burton E. Livingston, Professor of Plant Physiology, Johns Hopkins University.

Clarence E. McClung, Director of Zoölogical Laboratory, University of Pennsylvania.

Henry F. Moore, Deputy Commissioner, U. S. Bureau of Fisheries.

Ernest F. Nichols, professor of Physics, Yale University.

F. R. Moulton, Professor of Astronomy, University of Chicago.

Henry N. Russell, Professor of Astronomy, Princeton University.

Wallace C. Sabine, Hollis Professor of Mathematics and Natural Philosophy, Harvard University.

Frank Schlesinger, Director, Allegheny Observatory.

Raphael Zon, Chief, Office of Forest Investigation, U. S. Forest Service.

Rufus Cole, Director, Hospital of Rockefeller Institute.

The Chairman of the Council outlined the financial needs of the various Divisions of the Council, referring particularly to proposed activities of the Division of Engineering and of the Division of Agriculture, Botany, and Zoölogy and emphasized the necessity for the preparation of a budget for the Council. Upon motion, the Chairman was requested to appoint a special committee to construct such a budget, with instructions to confer with the Chairman of Divisions relative to respective needs, and to report at the next meeting of the Interim Committee, the Chairman of the Council to be an ex officio member. The other members of the Committee were appointed by the Chairman as follows: Arthur A. Noyes, Chairman, Whitman Cross, John C. Merriam.

The meeting adjourned at 11.50 a.m. with the understanding that the next meeting of the Executive Board will be held as authorized on Tuesday, June 11, 1918, at 9.30 a.m.

JOHN JOHNSTON, *Secretary.*

## *REPORT OF THE ANNUAL MEETING*

### Prepared by the Home Secretary

The Annual Meeting of the Academy was held in the Smithsonian Institution in Washington, April 22, 23, and 24, 1918.

Fifty-eight members were present as follows: C. G. Abbot, Ames, Bailey, Becker, Benedict, W. W. Campbell, Carty, F. W. Clarke, J. M. Clarke, Conklin, Crew, Cross, Dall, Davenport, Davis, Donaldson, Fewkes, Flexner, Hale, Halsted, Hillebrand, Howard, Howell, Iddings, Kasner, Lillie, Meltzer, Mendel, T. C. Mendenhall, Merriam, Michelson, Millikan, E. S. Morse, Moulton, E. L. Nichols, E. F. Nichols, A. A. Noyes, W. A. Noyes, H. F. Osborn, Pearl, Pupin, Ransome, Reid, Rosa, Schlesinger, Erwin F. Smith, Stratton, Thomson, Ulrich, Van Hise, Van Vleck, Walcott, Webster, Welch, Wheeler, D. White, Wilson, Woodward.

### BUSINESS SESSIONS

The President announced that the Home Secretary, Mr. Arthur L. Day would not be present, owing to absence from the city on matters relating to war.

#### REPORTS FROM OFFICERS OF THE ACADEMY

The annual report of the President to Congress for 1916 was presented and distributed.

The President presented the following communication from Mr. R. S. Woodward, resigning the chairmanship of the Committee on the Barnard Medal:

April 9, 1918.

*The Home Secretary,*
Dear Sir:

Permit me to tender through your office to the Council of the National Academy of Sciences my resignation as Chairman of the Committee on the Barnard Medal. Like our late colleague, Profesor William James, I find that with advancing years it is desirable to relieve oneself of what he fittingly called "life's baggage." Having assisted at the birth of many of our American Societies and served them in one way or another for many years, and having acted as the Chairman of the Committee on the Barnard Medal for a decade, I am disposed to think it will be advantageous for me, for the Academy, and for contemporary society to transfer the duties of the Chairmanship of the Committee in question to a younger man.

Faithfully yours,

Robert S. Woodward.

Moved: That the resignation of Mr. Robert S. Woodward as Chairman of the Committee on the Barnard Medal be accepted with regret, that Mr. T. H. Morgan be elected a member of the Committee, and that Mr. A. A. Noyes be elected Chairman. (Adopted.)

The President announced the following changes in the personnel of the Sections and Committees:

Section of Geology and Paleontology: J. M. Clarke, Chairman.

Section of Botany: Erwin F. Smith, Chairman.

Section of Anthropology and Psychology: J. WALTER FEWKES, Acting Chairman.

Local Committee: WHITMAN CROSS, Chairman, DAVID WHITE, FREDERICK L. RANSOME, RAYMOND PEARL, EDWARD O. ULRICH.

Other committee appointments with dates of expiration, were as follows:

Program: E. L. THORNDIKE to serve unexpired term of J. MCKEEN CATTELL 1920.

Henry Draper Fund: GEORGE E. HALE, to succeed himself, 1923.

J. Lawrence Smith Fund: E. S. DANA to succeed himself, 1923.

Comstock Fund: E. L. NICHOLS to succeed himself as member and Chairman ,1923.

Marsh Fund: CHARLES SCHUCHERT to succeed himself, 1921.

Murray Fund: WILLIAM H. DALL to succeed himself as member and Chairman, 1921.

Marcellus Hartley Fund: HENRY F. OSBORN and MICHAEL I. PUPIN to succeed themselves, 1921.

Barnard Medal: A. A. NOYES, Chairman; T. H. MORGAN to succeed R. S. WOODWARD.

The report of the Treasurer was presented in printed form and approved.

The report of the Home Secretary was presented as follows:

April 22, 1918.

*The President of the National Academy of Sciences.*

SIR: I have the honor to present the following report on the publications and member ship of the National Academy of Sciences for the year ending April 24, 1918.

No Scientific Memoirs have been published during the year, but "The Complete Classification of the Triad Systems in Fifteen Elements," by H. S. White, F. N. Cole and Miss L. D. Cummins, recommended to be published as one of the Memoirs of the National Academy of Sciences, was approved by the Committee on Publication and is now in the hands of the Public Printer. The actual publication may be delayed, owing to war conditions.

Of the Biographical Memoirs, that of John Shaw Billings, by S. Wier Mitchell, with the Scientific Work of John Shaw Billings, by Fielding H. Garrison; and William Stimpson, by Alfred G. Mayer, have been published. The former has been distributed, while that of Stimpson has just come from the printer. The manuscript of the Biographical Memoir of James Wright Dana, by L. V. Pirsson, Cleveland Abbe, by W. J. Humphreys, and William Bullock Clark, by John M. Clarke, are in the hands of the Committee on Printing; that of Benjamin O. Peirce, by Edwin H. Hall, is now in galley proof.

The Annual Report for 1917 has been published and is ready for distribution. The Proceedings of the Academy have been published regularly and have reached the third number of the fourth volume.

Four members have died since the last meeting. William Bullock Clark, elected in 1908, died July 27, 1917; James M. Crafts, elected in 1872, died June 21, 1917; Arnold Hague, elected in 1885, died May 15, 1917; Franklin Paine Mall, elected in 1907, died November 17, 1917, and one foreign associate, Adolf von Baeyer, died in August, 1917.

Fifteen new members were elected in April, 1917, making 157 active members on the present membership list; one honorary member and 36 foreign associates.

ARTHUR L. DAY, *Home Secretary.*

### REPORTS FROM COMMITTEES ON TRUST FUNDS

A report was received from the Directors of the Bache Fund, signed by Edwin B. Frost (Chairman), stating that since the annual meeting of the Academy in April, 1917, grants Nos. 205–209 (as announced in the PROCEEDINGS, p. 273, below) had been made; and that reports on these and previous grants had been received as follows:

No. 193, C. A. KOFOID, University of California, for assistance in securing animals in the Indian jungle and in their preparation for study in research on the intestinal protozoa. Mr. Kofoid is serving as Major in the Sanitary Corps and the work is being continued by research assistants on the material collected during Mr. Kofoid's travels.

No. 202, W. C. ALLEE, Lake Forest College, Illinois. Research concluded on 'The salt content of natural waters in relation to rhectaxis in Asellus," *Biol. Bull., Woods Hole,* **32,** 93–971. Preliminary results on first phases of investigation concerning a possible correlation between $CO_2$ production and phototaxis are completed and are in the hands of the editors of *J. Exp. Zool., Philadelphia.* Research is in progress concerning the cause of formation of aggregations of may-fly nymphs and other reactions, and to $CO_2$ production. Also effect of molting on both above factors; also as to effect of the cyanides on respiration and reactions to various stimuli.

No. 203, J. P. IDDINGS, Brinklow, Maryland. The chemical analysis of igneous rocks collected in Asia and Australasia is completed and the results published in the PROCEEDINGS of this Academy. The work with the microscope and thin sections will continue for some years to come.

No. 204, IRVING W. BAILEY, Bussey Institution, Harvard University, for field data for a study of environment upon the anatomical structure of angiosperms. Cancelled and the money returned to the Treasurer because it was found impracticable for Professor Bailey to go to Guatemala under present war conditions.

No. 205, T. H. GOODSPEED, University of California. Between 20,000 and 25,000 measurements in the flower-size investigations on Nicotiana were made duringt he summer and fall of 1917. These experiments are to be concluded during the summer and results published thereafter.

No. 206, REGINALD A. DALY, Harvard University. The deep sea thermograph has been completed and a description has been published by its designer, Mr. Harry A. Clark, in *Bull. Mus. Comp. Zool., Harvard College,* **61,** No. 15.

No. 209, CECIL K. DRINKER, Harvard Medical School. Research still in progress. A paper, entitled "The factors concerned in the appearance of nucleated red blood corpuscles in the peripheral blood. II. Influence of procedures designed to increase the rate of blood flow through the blood-forming organs—hemorrhage and infusion," by Cecil K. Drinker Katharine R. Drinker and Henry A. Kreutzmann, was published in *J. Exp. Med.,* **27,** 383–397, 1918.

The Directors have voted to transfer to capital account the sum of $2,575, which has been carried for a number of years as 'invested income,' together with $425 of current income, so that the principal of the fund now stands at $59,000.

An effort is being made to pay the American collaborators of the 'Nomenclator Animalium Generum et Subgenerum' the amounts due them for their work. A grant of $1,000 was made in 1913 to Professor F. E. Schulze, of Berlin, specifically for assistance by American men of science in this undertaking It now seems probable that these men can soon receive their compensation. After providing for this liability of $1,000, the cash on hand available for grants. on April 15, 1918, is $10.81.

A report was received from the Trustees of the Watson Fund, signed by A. O. Leuschner (Chairman), W. L. Elkins, and G. C. Comstock, stating that grants Nos. 16–17 (as announced below, p. 273) were recommended. Reports on previous grants were as follows:

No. 14, John A. Miller, Sproul Observatory, Swarthmore College, is expending his appropriation for the services of an assistant in parallax determinations. A list of 50 parallaxes, together with a list of observations and reductions, has been published. A second list of 50 should be ready before the beginning of January, 1919.

No. 15, Herbert C. Wilson, Goodsell Observatory, Northfield, Minn., is expending the appropriation for the services of an assistant in measuring and reducing photographic plates of asteroids for position and magnitude. The measurement and reduction of 97 plates has been completed. For 71, the results are published in *Pub. Goodsell Obs., Northfield, Minn.*, No. 5. Of the 26 unpublished measures, 15 are of plates of seven Watson Asteroids. A highly satisfactory color screen suitable for the 16-inch telescope has been made by R. J. Wallace, Research Laboratories of the Cramer Dry Plate Company.

A report was received from the Committee on the Henry Draper Fund, signed by W. W. Campbell (Chairman), as follows:

No awards have been made in the past year: the reduced size of many observatory and laboratory staffs, because of war activities, has not permitted normal development of new methods in astrophysics, and there have been no urgent calls for special instruments.

A grant of $500 made to W. W. Campbell two years ago, to provide the mounting of an ultra-violet spectrograph for use with the Crossley reflecting telescope, was expended, and the finished instrument was described in *Lick. Obs. Bull., Berkeley*, No. 291, in May, 1917. This instrument has been used by Mr. Wright in photographing the spectra of all the brighter planetary nebulae in the northern two-thirds of the sky.

A grant of $300 made in April, 1917, to Professor Joel Stebbins, of the University of Illinois, and a similar grant in like amount made earlier, are being applied, in accordance with agreement, in payment of part-time salary of an assistant to Dr. Stebbins in order to further the latter's development of the photo-electric cell photometer and the application to the study of variable stars. Quoting from Professor Stebbins' report dated April, 1918: "The photo-electric photometer has been improved until it is now from ten to twenty times as sensitive as the selenium photometer. We can measure the brightness of stars three magnitudes fainter than those which it was possible to observe with the selenium instrument. About a dozen new bright variable stars have been discovered, of which eight have been announced at meetings of the American Astronomical Society. We are also carrying on studies of several known variables." In further agreement with the terms of the grant, the University of Illinois has arranged to continue the assistant on full time in Professor Stebbins's department after the close of the present academic year.

A report was received from the Committee on the J. Lawrence Smith Fund, signed by E. W. Morley (Chairman), stating that the Fund now has a cash balance of income of $1188.85 and a balance of invested income amounting to $1532.50, recommending grants No. 9 (as announced below, p. 274), and containing the following reports on previous grants:

No. 3, Edmund O. Hovey, American Museum of Natural History, New York, received in 1909 a grant of $400 to aid in the study of certain meteorites. The pressure of adminstrative duties has been so severe that the investigation is suspended, at least for a time, and Dr. Hovey proposed to return the grant. The Committee hesitated, but at last accepted the proposal, and the amount of the grant, together with interest, has been repaid, to the Treasurer of the Academy.

No. 4, C. C. TROWBRIDGE, Columbia University, New York, has received from 1909 to 1917, grants amounting to $1400 to aid in the study of meteor trains. The work of the last year has consisted of the further collection of illustrations of meteor trains and the study of spectrum observations. No further grant is sought. A letter from the recipient follows.

*Dear Professor Morley:*

I beg to submit herewith a brief report relative to the grant made to me from the J. Lawrence Smith Fund for aid in work on the atmosphere of the earth and on the trains of meteors.

The progress for the year 1917 has been as follows: Work has been continued on the collection of illustrations of meteor trains; on the study of meteor train theories, and study of the spectrum observations.

It has been shown that there is good reason to believe that the spectra of trains observed indicate a gas phosphorescence similar to that of 'active' nitrogen (nitrogen in the phosphorescent state) rather than the light from hot metals as believed by the various observers. Further observations of importance have been made on the auroral features of the meteor train zone. This phase of the investigation is bound up with meteor train study for the following reasons:

The meteor train zone and the main aurora are the same; the potential gradient across this zone is probably the cause of the visual effects in both cases. The drift of the atmosphere as shown by drifting trains in the meteor train zone unquestionably has an important bearing on the electric discharges which cause the auroral glow.

*Financial Statement*

| | |
|---|---|
| Total funds received to December 31, 1916 | $1,153.50 |
| Total expenditures to December 31, 1916 | 1,020.67 |
| Balance, December 31, 1916 | $132.83 |
| (as per financial statement submitted March 26, 1917) | |
| Received from Treas., Nat. Acad. Sciences, March 22, 1917 | 250.00 |
| Total | $382.83 |

*Expenditures in 1917*

| | | | |
|---|---|---|---|
| January | | M. M. King, typewriting | $2.40 |
| February | | Science reprints | 8.00 |
| March | | Exchange on Washington check | .10 |
| March | 12 | Geo. Merritt, Jr., asst. 23 hours | 13.80 |
| April | 2 | J. Boldtman, photographer | 1.50 |
| April | 2 | Bureau of Supplies, Columbia Univ. | 1.20 |
| April | 30 | J. Boldtman, photographer | 3.40 |
| May | 1 | Geo. Merritt, Jr., assistant | 19.60 |
| May | 10 | Bur. of Supplies, Columbia Univ. | 1.60 |
| June | 1 | Amy E. Davis, assistant | 36.00 |
| June | 15 | Mabel Weil, assistant | 62.70 |
| June | 18 | Printing, New Era Printing Co. | 5.89 |
| | | | $156.19 |

*Cash on hand, December 31, 1917*

| | |
|---|---|
| Balance in Corn Exchange Bank | $149.90 |
| Cash | 1.45 |
| Balance in Washington Savings Bank | 75.29 |
| | $226.64 |

*Condensed Statement*

| | | |
|---|---|---|
| Total funds received | | $1,403.50 |
| Total expenditures to December 31, 1917 | $1,176.86 | |
| Balance | 226.64 | 1,403.50 |

Respectfully yours,

C. C. Trowbridge.

No. 6, S. A. Mitchell, University of Virginia, University, Virginia, received in 1915 a grant of $500 to aid in securing observations of paths and radiants of meteors, and in computing orbits when the observations are sufficient; further grants of $300 were made in 1916 and 1917. The war lessened the number of observations secured, during 1917, but this lessened number amounted to 4,231, making the whole number of observations about 19,000. An abstract of results for 1917 has been published by Dr. Olivier in a recent number of *Popular Astronomy.*

No. 7, George P. Merrill, United States National Museum, received last November a grant of $400 to aid in further study of certain elements in meteorites. Of this grant he has spent $185, and there remains to his credit $215.

A report was received from the Directors of the Wolcott Gibbs Fund, signed by C. L. Jackson (Chairman), stating that during the past year no application was received for a grant from the Fund, and owing to the disorganization of chemical research by the war, it was not thought worth while to try to get applications; that the unexpended income of the Fund amounts to $360.87; and that holders of grants have reported as follows:

Nos. 2 and 5, M. E. Holmes, of Connecticut College, finds that the exacting duties of her new position will prevent her from continuing her research, and has returned the unexpended balance of the two grants amounting to $86.32.

No. 6, G. P. Baxter, Harvard University, has spent $194.31 for material and apparatus since the last report. No platinum was bought, as the war had rendered it too expensive. The three researches described in the last report have been finished, and accounts of the results will be published soon. These were (*a*) Electrolytic Deposition in a Mercury Cathode, and the Atomic Weight of Cadmium, with C. H. Wilson. A slight loss was discovered, and corrected experimentally, after which eight determinations gave the atomic weight of cadmium as 112.40. (*b*) Impurities in Silver and Iodine, with L. W. Parsons. In 1 gram of the silver used for atomic weight determinations 0.004 mgm. of gas were found; in the iodine, 0.002 mgm. of gas. Most exacting spectroscopic tests showed no impurities in the silver. These results dispose of the criticisms of P. A. Guye on the Harvard atomic weight determinations. (*c*) The ratio of Arsenic Trioxide to Iodine, with L. A. Youtz. The atomic weight, 74.96, was found for arsenic. In addition a fourth research has been started. (*d*) The Atomic Weight of Mercury, with Matsusuke Kobayashi. All these researches but the last have been stopped by the enlistment of the graduate students in war service.

Nos. 4 and 7, W. D. Harkins, University of Chicago, has bought a vacuum pump for $55.00, and is about to buy one still more efficient. He has studied the "Secondary Valence and Werner's Co-ordination Number from the Standpoint of the Ammines of Cobalt" with G. L. Clark. The existence of the salt $CoCl_2IONH_3$ has been established; and the phenomena observed in the formation of this compound have been studied, and some of its physical constants. The work was broken off by the enlistment of Mr. Clark in the service of the Government before it could be brought to an end. It will be continued as soon as possible, as it promises to show either that Werner's constitution of the cobaltammines is incorrect, or that his co-ordination number must be changed from 6 to 10. Other results of great interest are also indicated.

No. 8, R. L. Datta, Presidency College, Calcutta, has sent orders for organic chemicals amounting to £20, but has received none because of the war.

A report was received from the Committee on the Marsh Fund, signed by E. H. Moore, Chairman, recommending grant No. 2 (as announced below, p. 274) and containing the following statement:

No. 1, J. M. Clarke, State Museum, Albany, reports progress in the assembling of material for his study of mutualism, symbiosis, and dependent life among animals.

A report was received from the Committee on the Murray Fund, signed by Wm. H. Dall, Chairman, as follows:

On November 1, the sum to the credit of the Murray Fund was $569.61. The Treasurer suggested and as Chairman I approved of the investment of the major part of this sum in Liberty bonds, which (should the Council decide to offer the medal this year) could readily be turned into cash. . . . .

A report was received from the Committee on the Comstock Fund, signed by Edw. L. Nichols, Chairman, as follows:

Five years having nearly elapsed since the first award of the Comstock prize to Professor (now Lieutenant Colonel) R. A. Millikan, the committee met in Philadelphia at the time of the November (1917) sessions of the Academy, four members being present. After careful consideration of various suggestions previously made in writing at the request of the chairman, the committee unanimously recommended that the second award of the Comstock Prize be made by the Academy to Samuel Jackson Barnett, Professor of Physics at Ohio State University, for his investigations on Magnetization by Rotation.

In this research the following fundamental and very important facts were established:

(1) that there are within the iron currents of negative electricity in orbital revolution,

(2) that these gyrostatic systems have inertia and are capable of definite orientation or arrangement by the rotation of the body without the action of any external magnetic field,

(3) that such orientation manifests itself as a magnetization of the body as a whole.

The committee further recommended that as in the case of the first award, the value of the prize be $1,500.00.

The President presented recommendations from the Committees on the Henry Draper Medal and the Daniel Giraud Elliot Medal and Honorarium for the following awards which were approved by the Academy:

American Museum of Natural History,
New York, February 7, 1918.

*My dear Mr. Day:*

Thanking you for your letter of February sixth and the return of my letter of January ninth, as Chairman of the Committee on the award of the Daniel Giraud Elliot Gold Medal, I believe that the committee will agree that Dr. Frank M. Chapman, of the American Museum of Natural History, in view of his memoir, "The Distribution of Bird-life in Colombia, a contribution to a biological survey of South America," *Bulletin of the American Museum of Natural History*, Volume XXXVI, 1917, pages 7–10 (Roman), 1–729 (Arabic), is entitled to first consideration and premiership for the year 1917, as having produced the most earnest single work in zoölogy, the result of six years serious exploration and research, namely, from December, 1910, until the publication of the volume in December, 1917.

As chairman of the committee, I respectfully submit this as a substitute report, with the approval of my colleagues.

Fortunately, Doctor Chapman is one of the most distinguished of the many followers of Dr. Joel Asaph Allen, and he was also closely associated with Dr. Daniel Giraud Elliot; consequently I trust the Committee will regard the award of the medal as eminently appropriate.

Respectfully submitted,

HENRY FAIRFIELD OSBORN, *Chairman.*

---

Mount Hamilton, January 19, 1918.

*Dear Dr. Day:*

I have just sent you the following night letter:

> "I herewith certify nomination of Walter S. Adams, Mount Wilson Observatory, for his investigations in astrophysics, by Draper Committee for award of Draper Gold Medal. I hope Council can make award practicable at this year's meeting."

Dr. Walter Sidney Adams has discovered and developed a method of determining the distances of the stars, by means of the spectrograph, which the Henry Draper Committee believes is one of the great advances in modern Astronomy. Like most methods of high value, its fundamentals are exceedingly simple. The basic fact is that certain lines are enhanced in the spectra of stars of high absolute luminosity, and certain other lines are enhanced in the spectra of stars of low absolute luminosity; in other words, the intensities of certain lines increase continuously with increasing absolute luminosities, and the intensities of certain other linds increase continuously with decreasing absolute luminosities. The approximate distances of a few hundred of the nearer stars, measured by the older methods, are known. These distances enable us to compute the absolute magnitudes or luminosities of those stars. The correlation of these absolute magnitudes and the estimated relative intensities of the critical lines in their spectra have given Mr. Adams the power to determine the functional relations connecting these elements, and thus to determine the absolute magnitudes of stars not yet observed for distance. Knowing the magnitude of a star as observed from the earth, and the absolute magnitude of the same star as determined by the spectrograph, the solution of a simple equation yields at once the value of the star's distance. Here is a powerful means of extending the list of stars whose distances are known, and the Committee confidently expects that the method will be applied successfully to thousands of stars which are too distant for successful attack by the earlier methods. The method should in due time tell us much concerning the linear scale of the nearer parts of our stellar system. The secondary applications of the method to astronomical problems are likewise of great promise.

Mr. Adams has applied the spectrographic method to the determination of the distances of more than five hundred stars, of the Harvard spectral classes F to Mb inclusive, and a comparison of his results with those obtained by the older methods, as to rapidity and accuracy, is a subject for congratulation.

The method has not yet been applied satisfactorily to stars of certain spectral classes, but it is hoped that the dependence of such spectra upon absolute luminosities or other elements may be found in the near future.

Dr. Adams's contributions to other lines of astronomical research have been many, extensive and fruitful. We mention three:

The spectrographic determination of the sun's rotational speeds by comparing the motions of approach of the eastern limb and the motions of recession of the western limb with the apparent motions of approach or recession of points on the solar meridian passing through the earth. The results form a monumental contribution to the complicated and still unsolved problem of the solar rotation.

His long exposures on the spectra of the present-day faint remnants of new stars formerly brilliant have shown that these stars are now of the Wolf-Rayet type.

The more thorough application of high dispersion spectroscopy to the study of the sun's chromosphere. Prior to the establishment of the Mount Wilson Solar Observatory, this was essentially a total solar eclipse problem. Stratification of the chromosphere has been studied by Adams without an eclipse at least as successfully as it had formerly been studied at times of total solar eclipse.

Respectfully submitted,

W. W. CAMPBELL, *Chairman.*

## GENERAL BUSINESS

The following report from the Editorial Board of the PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES was presented by the Chairman, Raymond Pearl:

1. Three volumes of the PROCEEDINGS have been completed, and four numbers of the fourth volume have been issued.

The statistics as to the make-up of the third volume, both in respect of subject matter and of source of the contributions, have been printed in the Annual Report of the Academy for 1917, and need not now be repeated except so far as covers one point.

The statistics of articles by members of the Academy as compared with articles by non-members are interesting mainly in showing a progressive diminution in the percentage of articles by members, despite the increase in membership of the Academy. If there are obstacles which can be removed and which hinder members of the Academy from printing in the PROCEEDINGS, would it not be well to make efforts to remove them? The Academy represents the highest point in American research, and if the PROCEEDINGS should actually contain articles representing the totality of the investigations of members of the Academy it would become thereby largely representative of all American research and of very high grade, and furthermore it would be more truly the Proceedings of the Academy in the sense that corresponding publications of foreign academies are representative of their research.

2. At the Autumn meeting the terms of office of five members of the Editorial Board expired, and new appointments were made by the Council as follows: Jacques Loeb, W. M. Wheeler, E. B. Frost, E. L. Thorndike, and E. H. Moore.

3. At the Autumn meeting the Board decided to put into operation certain changes in the typographical make-up of the PROCEEDINGS in the interest of economy. These changes have been made with satisfactory results.

4. The Editorial Board is of the opinion that in view of the now established and recognized position of the PROCEEDINGS as a medium of scientific publication, the members of the Academy might well contribute more of their own papers to its pages than they now do, both from the standpoint of self-interest as well as from a sense of duty to the Academy and what it stands for. In this connection the Board would recommend that the Academy adopt as a general principle the policy of requiring each recipient of a grant for research from any of its special funds to publish some account of the results of the researches under the grant in the PROCEEDINGS.

5. If the above recommendation is adopted, the Board would further recommend that the Academy suggest to the several committees having in charge trust funds from which grants are made that whenever accounts of researches under grants are published in the PROCEED-

INGS there shall be paid over from the Trust Funds out of which the grants are made, to the PROCEEDINGS account, if such action be permissible under the terms of the bequest, a sum of money to cover the expense of the publication at a rate of $6.00 per printed page.

Anent the above report the following recommendations were submitted from the Council and adopted.

That the following recommendations from the Editorial Board of the PROCEEDINGS be approved by the Academy and that the Home Secretary be instructed to bring these recomdations to the attention of the members of the Academy and the chairmen of the trust funds.

That members of the Academy be requested to contribute their own papers to the PROCEEDINGS.

That the policy of requiring each recipient of a grant for any research from any of the special funds to publish an account of the results of the researches under the grant in the PROCEEDINGS be approved.

That the Academy request the committees and trustees of the several trust funds of the Academy from which grants are made that whenever accounts of researches under grants are published in the PROCEEDINGS there shall be paid over from the Trust Fund out of which the grants are made, to the PROCEEDINGS account, if such action is permissible under the terms of the bequest, a sum of money to cover the expense of the publication.

A report was received from the Finance Committee of the PROCEEDINGS, signed by C. B. Davenport, Chairman, F. R. Lillie, and Raymond Pearl, as follows:

The estimated net cost of the PROCEEDINGS for 1918 is $5,600.00.

The estimated income is as follows:

| | |
|---|---|
| From subscriptions (provided each member of the Academy becomes responsible for one subscription) | $1,800.00 |
| One-third guarantee fund of $2,500 | 833.00 |
| Estimated income of Billings Fund | 187.00 |
| Sundry other income (members dues, $850, N.R.C., $400, Dr. Walcott, special, $100) | 1,350.00 |
| Total estimated income | $4,170.00 |
| Total estimated deficit | $1,430.00 |

If recommendation of the Editorial Board that space for reports of special grants in PROCEEDINGS be specially paid for be adopted, this deficit will be reduced to $1,200.00.

The Committee plans to raise funds to meet this deficit.

A report was received from the Auditing Committee, signed by C. G. Abbot, Chairman, W. F. Durand, and A. L. Day, certifying the accuracy of the Treasurer's books.

The Chairman of the Research Council, Mr. Hale, presented a verbal report in which he called attention to the printed report of the Research Council which had appeared in the Annual Report of the National Academy of Sciences.

The following amendment to the Constitution was presented from the Committee of the Whole and adopted:

That the constitution be amended by substituting in Article II, Section 1, line 3 (Report, 1916) the word *four* for the word *six* so that it will read for a term of four years . . . .

to take effect on the expiration of the term of office of the present incumbents or in case of a vacancy.

Article II, Section 1, when amended to read:

SECTION 1. The officers of the academy shall be a president, a vice president, a foreign secretary, a home secretary, and a treasurer, all of whom shall be elected for a term of four years, by a majority of votes present, at the first stated meeting after the expiration of the current terms, provided that existing officers retain their places until their successors are elected. In case of a vacancy, the election for four years shall be held in the same manner at the meeting when such vacancy occurs, or at the next stated meeting thereafter, as the academy may direct. A vacancy in the office of treasurer or home secretary may, however, be filled by appointment of the president of the academy until the next stated meeting of the academy.

The President, in closing, gave the present status of the aeronautical work that is being carried on by the Academy for the country.

## ELECTION OF OFFICERS AND MEMBERS

*Two Members of the Council.*—W. H. HOWELL to succeed himself, term expiring in 1921; C. G. ABBOT to succeed John M. Coulter, term expiring in 1921.

The following persons were elected as new members of the Academy.

ROBERT GRANT AITKEN, Astronomer, Lick Observatory, California.
GEORGE FRANCIS ATKINSON, Botanist, Cornell University, Ithaca, New York.
GEORGE DAVID BIRKHOFF, Mathematician, Harvard University, Cambridge, Mass.
PERCY WILLIAMS BRIDGMAN, Physicist, Harvard University, Cambridge, Mass.
STEPHEN ALFRED FORBES, Zoölogist, Urbana, Illinois.
JOHN RIPLEY FREEMAN, Engineer, Providence, Rhode Island.
CHARLES JUDSON HERRICK, Neurologist, University of Chicago, Chicago, Illinois.
LUDVIG HEKTOEN, Pathologist, University of Chicago, Chicago, Illinois.
FRANK BALDWIN JEWETT, Engineer, Western Electric Company, New York, New York.
WALTER JONES, Physiologist, Johns Hopkins University, Baltimore, Maryland.
IRVING LANGMUIR, Chemist, General Electric Company, Schenectady, New York.
CHARLES ELWOOD MENDENHALL, Physicist, University of Wisconsin, Madison, Wisconsin.
JOHN CAMPBELL MERRIAM, Paleontologist, University of California, Berkeley, California.
HENRY NORRIS RUSSELL, Astronomer, Princeton University. Princeton, New Jersey.
DAVID WATSON TAYLOR, Engineer, Admiral and Chief of the Bureau of Construction and Repair, United States Navy.

## SCIENTIFIC SESSIONS

Two public lectures on the WILLIAM ELLERY HALE Foundation were given on April 22 and 23 by JOHN C. MERRIAM, of the University of California, on the Beginnings of Human History from the Geologic Record.

Four public scientific sessions were held on April 22 and 23 at which the following papers were presented:

FRANCIS G. BENEDICT: The effects of a prolonged reduced diet on twenty-five college men: I. On basal metabolism and nitrogen excretion.

WALTER R. MILES (introduced by F. G. Benedict): The effects of a prolonged reduced diet on twenty-five college men: II. On neuromuscular processes and mental condition (illustrated).

H. Monmouth Smith (introduced by F. G. Benedict): The effects of a prolonged reduced diet on twenty-five college men: III. On efficiency during muscular work and general muscular condition (motion pictures).

W. S. Halsted: The partial occlusion of great arteries in man and animals (illustrated).

S. J. Meltzer: Three papers (illustrated). (*a*) The favorable effect of subcutaneous injection of magnesium sulphate in tetanus. (*b*) The possible danger of intravenous injection of magnesium sulphate. (*c*) The antagonistic and curative action of calcium salts in these cases.

Henry Fairfield Osborn: The Liberty field hospital ward. Designed on the unit construction plan. Portable. Adapted to American overseas summer and winter service (motion pictures).

Simon Flexner: The war and medical research (illustrated).

Edward Kasner: Conformal geometry.

S. J. Barnett (by invitation. Comstock prize recipient): Magnetism by rotation.

A. A. Michelson: On the correction of optical surfaces.

W. W. Campbell: Some recent observations of the brighter nebulae (illustrated).

R. A. Millikan: Physical researches for the war.

F. W. Clarke: Notes on isotopic lead.

Lawrence J. Henderson (introduced by Raymond Pearl): The physico-chemical properties of gluten.

Thomas Wayland Vaughan (introduced by David White): Correlation of the tertiary formations of the southeastern United States, Central America and the West Indies.

W. M. Davis: Coast survey charts and fringing reefs of the Philippine Islands (illustrated).

Henry Fairfield Osborn and William K. Gregory: Recent researches on the skeletal adaptations and modes of locomotion of the Sauropod Dinosaurs (illustrated).

Charles D. Walcott: Some additional data on the Cambrian Trilobites (illustrated).

C. R. Van Hise: The development of Governmental regulations during the world war.

C. Hart Merriam: The big bears of North America.

G. H. Parker: The growth of the Pribilof fur-seal herd between 1912 and 1917 (illustrated).

Henry H. Donaldson: A comparison of the growth changes in the nervous system of the rat with the corresponding changes in man (illustrated).

Robert M. Yerkes (by invitation): Measuring the mental strength of an army (illustrated).

Arthur Gordon Webster: Some considerations on the exterior ballistics of a gun of 75 miles range.

J. P. Iddings: Biographical memoir of the late Arnold Hague.

C. G. Abbot: Perodicity in the variation of the sun.

John M. Clarke: Biographical memoir of the late William Bullock Clark.

Edwin H. Hall: Ionization in solid metals.

E. L. Nichols and H. L. Howes: On the types of decay of phosphorescence.

## AWARD OF MEDALS

The following medals were awarded at the annual dinner on the evening of April 23, 1918, at the Cosmos Club:

*The Comstock Prize* of $1500.00, to Samuel Jackson Barnett, of Ohio State University, for his investigations in magnetization by rotation.

*The Henry Draper Medal*, to Walter S. Adams, of the Mt. Wilson Solar Observatory, California, for his investigations in astrophysics.

*The Daniel Giraud Elliot Gold Medal and Honorarium*, to Frank M. Chapman, of the American Museum of Natural History, for his memoir, "The Distribution of Bird Life in Colombia, A Contribution to a Biological Survey of South America," *Bulletin of the American Museum of Natural History*, **36**, 1917, (vii–x, 1–729).

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## RESEARCH GRANTS FROM TRUST FUNDS OF THE ACADEMY

During the twelve months preceding the Annual Meeting of the Academy the following grants for the promotion of research were made from the Trust Funds of the Academy.

### GRANTS FROM THE BACHE FUND

No. 205, T. H. Goodspeed, University of California, $100. For studies of inheritance in Nicotiana hybrids.

No. 206, Reginald A. Daly, Harvard University, $700. For the completion of the deep sea thermograph designed and partly constructed under Grant No. 194. In continuation of No. 194.

No. 207, T. H. Gronwall, New York City, $300. To complete and extend mathematical researches on conformal representation.

No. 208, A. Franklin Shull, University of Michigan, $400. To investigate the cause of sex production and the life cycle of rotifers, together with artificial modification of life cycle; differential factors in fertilization of male-producing and female-producing rotifers; sex determination and the life cycle of the thrips; cause of sex production, wing production, and other cyclical phenomena in aphids.

No. 209, Cecil K. Drinker, Harvard Medical School, $350. For the closer study of the factors involved in extension of unchecked red cells and leucocytes in the dog.

### GRANTS FROM THE WATSON FUND

No. 16, Herbert C. Wilson, Goodsell Observatory, $300. For a continuance of the work of the determination of the position and brightness of asteroids (chiefly those discovered by Watson) by the photographic method, together with a study of the brightness of some variable stars. (Supplementary to Grant No. 15).

No. 17, John A. Miller, Sproul Observatory, $500. To measure plates for determining stellar parallaxes (Supplementary to Grant No. 14).

### GRANTS FROM THE J. LAWRENCE SMITH FUND

No. 9. S. A. Mitchell, University of Virginia, $300. To continue his researches on the paths, radiants, and orbits of meteors. (Supplementary to Grant No. 8.)

### GRANT FROM THE MARSH FUND

No. 2, M. Ferdinand Canu, Versailles, France, $250. For investigations in cooperation with Dr. R. S. Bassler, of the United States National Museum, of the early tertiary bryozoa of North America.

## INFORMATION TO SUBSCRIBERS

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# PROCEEDINGS
OF THE
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OF THE
UNITED STATES OF AMERICA

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THE PROCEEDINGS is the official organ of the Academy for the publication of brief accounts of important current researches of members of the Academy and of other American investigators, and for reports on the meetings and other activities of the Academy. Publication in the Proceedings will supplement that in journals devoted to the special branches of science. The Proceedings will aim especially to secure prompt publication of original announcements of discoveries and wide circulation of the results of American research among investigators in other countries and in all branches of science.

ARTICLES should be brief, not to exceed 2500 words or 6 printed pages, although under certain conditions longer articles may be published. Technical details of the work and long tables of data should be reserved for publication in special journals. But authors should be precise in making clear the new results and should give some record of the methods and data upon which they are based. The viewpoint should be comprehensive in giving the relation of the paper to previous publications of the author or of others and in exhibiting where practicable, the significance of the work for other branches of science.

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REFERENCES to literature, numbered consecutively, will be placed at the end of the article and short footnotes should be avoided. It is suggested that references to periodicals be furnished in some detail and in general in accordance with the standard adopted for the Subject Catalogue of the International Catalogue of Scientific Literature, viz., name of author, with initials following (ordinarily omitting title of paper), abbreviated name of Journal, with place of publication, series (if any), volume, year, inclusive pages. For example: Montgomery, T. H., *J. Morph., Boston,* 22, 1911, (731–815); or, Wheeler, W. M., *Königsburg, Schr. physik. Ges.,* 55, 1914, (1–142).

PAPERS by members of the Academy may be sent to Edwin Bidwell Wilson, Managing Editor, Mass. Institute of Technology, Cambridge, Mass. Papers by non-members should be submitted through some member.

PROOF will not ordinarily be sent; if an author asks for proof, it will be sent with the understanding that charges for his corrections shall be billed to him. Authors are therefore requested to make final revisions on the typewritten manuscripts. The editors cannot undertake to do more than correct obvious minor errors.

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PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES

**Volume 4** SEPTEMBER 15, 1918 **Number 9**

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# *METALLIFEROUS LATERITE IN NEW CALEDONIA*

By W. M. Davis

Department of Geology and Geography, Harvard University

Communicated June 24, 1918

A recent paper on lateritic ore deposits by W. G. Miller[1] gives among other matters an account of the composition of the nickel- and cobalt-bearing laterite of New Caledonia, but does not call attention to the physiographic relations of the laterite, probably because the physiography of the island has been little discussed in published articles. Even the manifest evidence of submergence given by its embayed shore line has hardly been mentioned by the students of its geology. The mature sea cliffs, which usually cut off the hard-rock highlands along the northeastern coast and which frequently descend, except for narrow fringing reefs, into ten or twenty fathoms of water, contrast strongly with the rounded hills and sloping lowlands of weaker rocks that dip gradually under sea level along the southwestern coast; but the contrast has only been treated empirically if at all in accounts of the island. Furthermore, the form of the northeastern cliffs, the depth of the reef-enclosed lagoon in front of them, and still more the form of the embayed valleys that interrupt the cliffs, all taken together, indicate that the cliffs were cut by waves while the island stood several hundred feet higher than now, and that this higher stand occurred during a subrecent period of the physiographic development of the island when the northeastern coast must have been unprotected by coral reefs for a time long enough for the cliffs to be worn back several miles; but these physiographic contributions to the historical geology of the island, not being attested by fossiliferous stratified deposits, appear to have been overlooked. As long as elements so important as these in the historic geology of New Caledonia land forms remain unstated, the origin of its superficial ore deposits will necessarily be unsolved.

The most significant features in connection with the ore deposits are the highlands on which they lie. Although the greater part of the mountainous island is of irregular form and varying altitude, there are certain districts, particularly those occupied by serpentine rocks, which are characterized by rolling highlands of moderate relief at altitudes of 600, 800 or 1000 feet. These seem to be elevated peneplain areas; they are trenched by relatively steep-sided valleys, and are adjoined either by surmounting residual mountains, presumably formed of more resistant rocks, or by the lower hills and lowlands of the southwestern coast where weaker rocks prevail. The erosion of the subdued southwestern lowlands and of the narrow valleys in the highland areas has evidently been accomplished after the partial peneplanation of the island and its subsequent elevation, and during the same period of higher stand that witnessed the cutting of the sea cliffs along the northeastern coast, previous to the recent submergence by which the shoreline was embayed. The amount of the recent submergence may well have been from 600 feet or more; the previous upheaval of the northeastern side of the island, before its cliffs were cut, was probably at least twice as great, for the sea-cliffs today, in spite of being partly submerged, not infrequently still show 600 or 1000 feet of their height above water. The absence of all consideration of these inferences in the geological accounts of New Caledonia affords a striking illustration of the contrast between the older geological philosophy that based its theories only on the structure of rocks and their mineral and fossil contents, and the newer philosophy of geology which broadens the older one by adding thereto a reasonable consideration of surface forms and their evolution.

During my relayed trip around the island on three trading steamers, supplemented by local sail-boat excursions, in June and July, 1914, the rolling highlands were recognized as elevated peneplains at many points on both coasts. Where their vegetation is scanty, as is often the case, the soils of the highland slopes are laid bare in rain-washed gulleys which disclose their varied colors, dark or black at the surface and usually a strange mixture of vivid reds and ochres beneath. At certain points the open workings of the highland laterite mines were seen, and at one harbor where a steamer touched for an afternoon I had time to climb the slopes and inspect the excavations. The residual nature of the deposits was manifest enough. The boulders, referred to in Miller's paper as lying at the bottom of the loose deposits and as affording a rim or coating that is scraped off and added to the ore pile, are perhaps partly concretionary in origin, but some of them appeared to be incompletely decomposed rock kernels lying almost in place within a matrix of more disintegrated material. It is significant that the abundant hill-side detritus is not worked; ore of paying richness and quantity seems to be limited to the highland areas. It is further significant that, as is usual in such residual deposits, analyses of the highland ore deposits show a much higher percentage of nickel and cobalt than is found in the underlying serpentines.

In view of all this it seems reasonable to infer that the ore deposits are the result of surface enrichment by leaching and concentration during the later stages of the above-mentioned cycle of peneplanation, and that they have been undergoing removal rather than further accumulation and enrichment in the later cycle of erosion introduced by elevation; the removal thus initiated is still continued in spite of the still later subsidence.

The general sequence of changes by which the present form of the island has been evolved from a subcontinental land of Tertiary or earlier time may be outlined as follows. A composite land mass of large, perhaps continental, extent, consisting hereabouts of deformed crystalline and Mesozoic rocks, was eroded to mountainous or moderate relief, *AB*, in the background block of figure 1; it was then reduced in area by down-warping, probably in tertiary time, whereby the surviving land area must have gained an embayed shore line, *C*, *D*, as in block 2. If coral reefs had previously existed around the border of the larger land, they must have been drowned by rapid submergence, for the adjoining seas are very deep. New reefs may have been formed in the later stages of submergence, enclosing a lagoon, *C*.

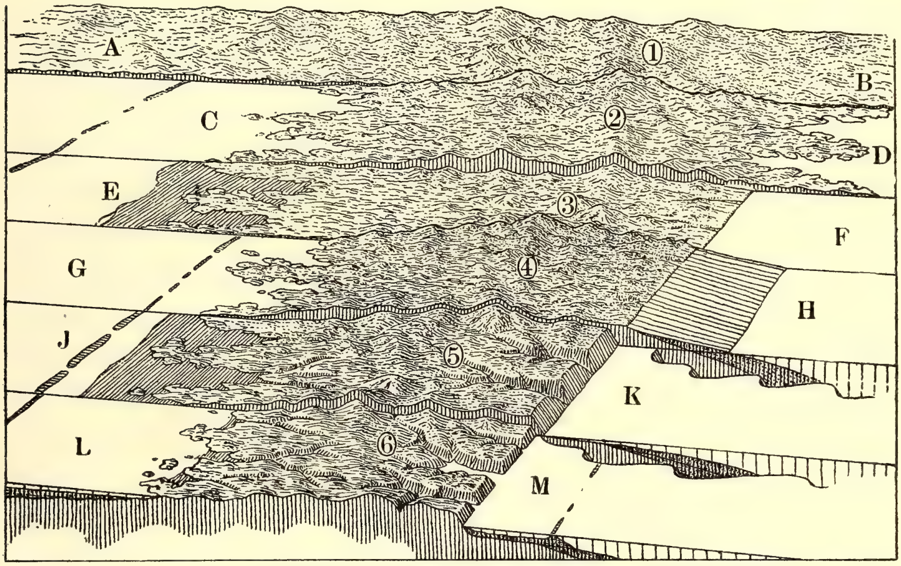

FIG. 1. THE PHYSIOLOGICAL DEVELOPMENT OF NEW CALEDONIA

The reduced island of block 2 must have stood still long enough to suffer reduction to small or low relief, except in its areas of most resistant rocks, as in block 3; the serpentine areas were mostly reduced to peneplains at this time. The embayments formed in the shore line at the beginning of this cycle of erosion were presumably in time filled with deltas that advanced into the reef-enclosed lagoon, as at *E;* the deltas may indeed have grown so far as to overwhelm and smother the reef, whereupon it would be cut away by the waves which would in time attack the worn-down land, retrograding its peneplains in low cliffs and spreading the detritus from them and the rivers on the shallow floor of the adjoining sea, *F;* for this change from a reef-fronted and prograded coast of submergence in an early stage of an erosion cycle that had been introduced by warping to a reef-free and retrograded coast in an old stage of the cycle is a most natural consequence of a long stationary period in the history of an island in the coral seas.

Another warping is then inferred, chiefly because the change from the subcontinental land of block 1 to a narrow island adjoined by deep seas on both sides, is not likely to have been accomplished in a single period of deformation. This warping must be supposed to have affected the island unsymmetrically, as in block 4, probably drowning any previously formed barrier-reefs along the southwestern coast, and re-embaying the shore line there, where a new barrier reef, *G*, would be developed, but this last point is not essential; at the same time the northeastern coast appears to have been uplifted, so that a coastal plain of marine sediments, such as may have accumulated in the shallow sea, *F*, was there added, as at *H*. The reason for the last inference is that the elevated peneplain areas along the northeastern side of the island were cut back in cliffs by the sea in the early stage of the cycle introduced by this elevation; and the simplest way of accounting for this is to suppose that the elevation here laid bare a narrow coastal plain, covered with loose sediments, on the shore line of which reef-building corals could not establish themselves, and on which the waves could therefore work unimpeded. No other supposition can so reasonably account for the abrasion of cliffs along one side of an island in the coral seas during the early stages of a subrecent cycle of erosion.

Block 4 is then gradually transformed into block 5, in which the weak-rock areas of the southwestern coast are again worn down to moderate relief, and the reef-enclosed lagoon is largely filled with delta plains, as at *J;* and in which the uplifted peneplains of stronger rocks along the northeastern coast are dissected by narrow valleys and cut back in high cliffs, as at *K*. A recent submergence has converted block 5 into block 6, drowning the previously developed delta plains of the southwestern coast, where the reef has grown higher and the sea has advanced farther than before on the lowland border, thus leaving the broad lagoon, *L*, of today between the young shore line of submergence and the barrier reef; the same recent submergence has partly drowned the cliffs of the northeastern coast, so that their valleys are now beautifully embayed, and a barrier reef has grown up from the sea bottom in front of them, as at *M*. It is chiefly upon the highland peneplains back of the cliffs of this coast, and upon similar highland areas which occur along the northwestern half of the other coast, that metaliferous laterites occur.

Abundant variations on the earlier stages of the foregoing scheme may be proposed. The changes here outlined are probably much simpler than the changes that have actually taken place, and some of the changes here indicated are very uncertain. For example the cutting back of the embayed coast, *D*, in block 2, to the low cliffs, *F*, of block 3, is by no means assured; but the unsymmetrical warping by which block 3 was transformed into block 4 seems to be essential as a means of reasonably providing for the development of sea cliffs in a relatively early stage of the cycle of erosion on one side of the island where the rocks are hard, while no cliffs are developed on the other side of the island, even though the cycle of erosion on the weaker rocks that there prevail advanced in the same measure of absolute time to a late stage of development, when reef extinction and retrogressive abrasion are expectable occurrences. The prevention of cliff development on the southwestern coast is favored by assuming that the submergence which lowered block 5 to block 6 was caused by progressive subsidence. In any case, the general submergence by which block 5 is changed to block 6 is easily demonstrated. It is therefore in view of some such geologically modern sequence of moderate deformation and prolonged erosion as is here sketched, uncertain and shadowy in its earlier stages, better certified in its later stages, that the development of the ore-bearing laterites must be explained.

The enrichment of the present ore deposits could not have begun on the serpentine areas in the immature stages of the earlier cycle of erosion in which block 2 was worn down to block 3, for at that time the valley-side slopes, profiles 1, 2, figure 2, were steep enough, just as they are in the immature stage, profiles 1′, 2′, of the present cycle in the serpentine areas, to allow the removal of disintegrated rock about as fast as it was weathered. Even during the mature stage, profile 3, of the earlier cycle, removal rather than accumulation must have prevailed; but as the later stages of the cycle were reached, soil removal from the subdued hills, profiles 4, 5, between the wide-open valleys must have been much slackened; the thickness of the disintegrated materials there occurring and with it the surface enrichment of the metallic ores by downward concentration must have thenceforward increased as the subdued hills were worn down to the gentler and gentler gradients of old age, as in profile 6; and the area on which concentration was important must have been on the low swells between the broad valleys. If these inferences are correct, it follows that the nickel and cobalt content of the greater part, *MNO*, of the primeval rock mass has been carried away and deposited on the adjoining sea floor, and that the deposits now worked contain chiefly the concentrated savings from only a quarter or a sixth of the primeval total, shaded in the upper half of figure 2. This explanation traverses Glasser's supposition[2] that the serpentine masses have not been much eroded; for in view of their form alone, apart from the evidence of erosion given by ore concentration, that supposition seems untenable.

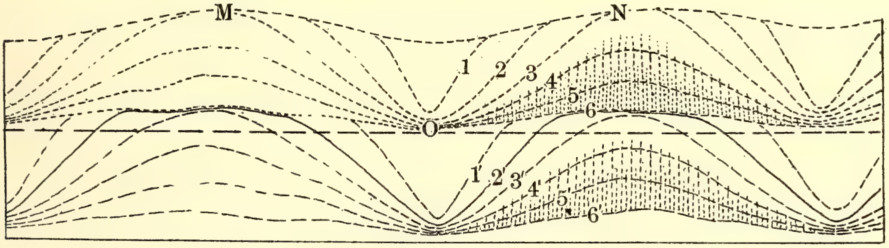

FIG. 2. RELATION OF ORE-BEARING LATERITE TO TWO CYCLES OF EROSION

Again, the submature or mature main valleys, profile 2′, of the prentcyclees presumably excavated beneath the wide-open old valleys of the earlier cycle, have not as yet encroached greatly upon the ore-bearing part of the inter-valley highlands, profile 6. The encroachment and removal will be greater and greater as the main valleys of the present cycle are widened, profile 3′, (the recent submergence is not indicated here) and as branch valleys are extended headward into the highland by retrogressive erosion. Still later, profile 4′, the highland surfaces of the earlier cycle and their residual laterite cover will be completely worn away; but finally, when old age is again approaching, profiles 5′ and 6′, new deposits will again be formed by rock disintegration and ore concentration on the subdued and lowering inter-valley hills of the future, just as happened in the past.

The superficial laterite ores of the serpentine highlands in New Caledonia therefore seem to be local as to area of development and intermittent as to time of origin and duration of occurrence. The same relations presumably obtain in a general way regarding the limonite and bauxite deposits of our Appalachian valleys.

[1] *Report, Ontario Bureau Mines*, No. 26, part 1, 1917.

[2] Richesses minérales de la Nouvelle Calédonie, *Ann. des Mines*, 1903–04.

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# *A COMPARISON OF GROWTH CHANGES IN THE NERVOUS SYSTEM OF THE RAT WITH CORRESPONDING CHANGES IN THE NERVOUS SYSTEM OF MAN*

By Henry H. Donaldson

Wistar Institute of Anatomy and Biology, Philadelphia

Read before the Academy, April 23, 1918

For a number of years the albino rat has been used for the study of growth changes which occur in the brain between birth and maturity.

As occasion offered, the results obtained from the rat have been compared with those from man, in order to determine how far the rat might be used for the study of the corresponding problems in man.

As all of these studies were in the field of growth, and as growth is a function of age, it became necessary in order to make the cross reference, to determine the equivalent ages of the rat and man.

Two observations were available for this determination.

1. The rat doubles its birth weight in 6 days, while man takes 180 days—giving a ratio of 1 to 30 days. From this it would appear that the rat was living 30 times as fast as man.

2. Again, a rat of 3 years is very old—so that I have ventured to compare a rat of this age with a man of 90 years. Once more the rat appears to be living 30 times as fast as man.

For working purposes we assumed that 1 day in the life of the rat was equivalent to 30 days in the life of man, and that the equivalent ages in these two animals were represented by equal fractions of the span of life.

One adjustment is necessary however in dealing with the data for the nervous system. The brain of the rat at birth is less mature than that of man at birth, and it is not until the rat is 5 days old that the brain is in the same phase as that of man at birth.

In making any comparison therefore the data for the rat at 5 days of age are arranged to coincide with the data for man at birth. Using the foregoing methods, four comparisons have been made between the growing nervous system of the rat and that of man.

The first chart is for the growth in the weight of the entire brain of the rat from birth to maturity, compared with that of man. When the human brain weights are reduced, and the comparison is made in the way described, the two graphs run well together. Thus, at equivalent ages, the brain in these two forms has undergone nearly the same degree of enlargement.

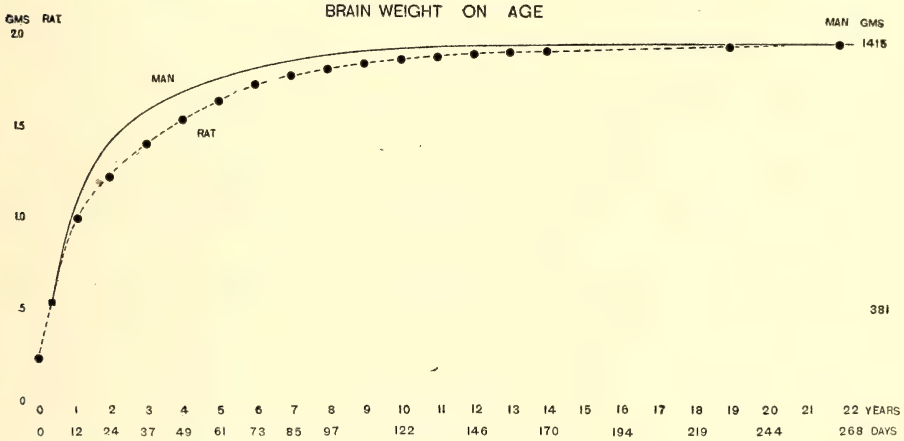

CHART 1

Showing the increase in the brain weight of the albino rat on age (broken line and dots) and compared with this the increase in the brain weight of man at the equivalent ages (solid line). The values for equivalent ages are on the same ordinate.

Chart 2 shows the percentage of water in the rat's brain at different ages. The graph indicates a rapid, followed by a slow loss of water, with advancing age. I have found only four corresponding records for man, namely at birth, 2 years, 9.5 years and 25 years, and these are entered by the heavy black dots at the equivalent ages on the graphs for the rat. The coincidence is good.

It has been determined that this loss of water is due to the progressive accumulation of myelin in the nervous system (Donaldson, '16) and the inference is therefore justified that the formation of myelin is progressing in a like manner in the two forms—only it progresses 30 times as fast in the rat.

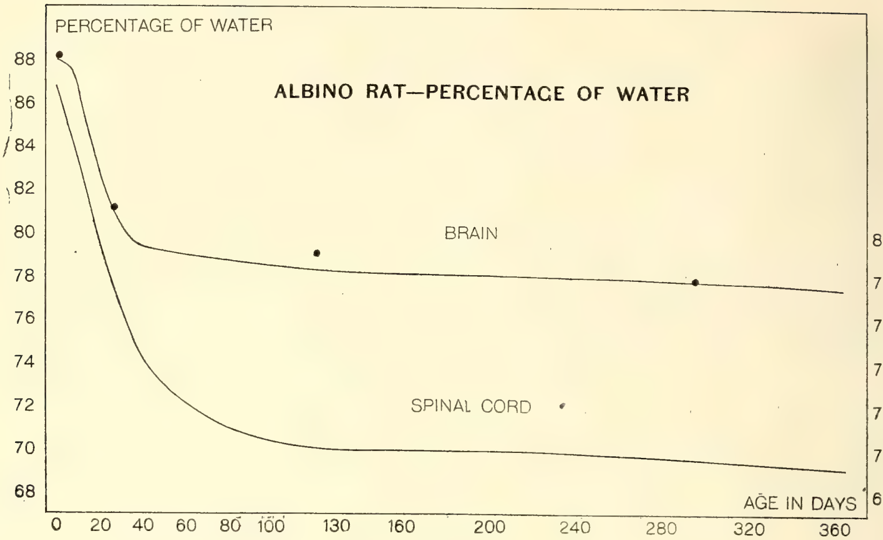

CHART 2

Showing on the upper graph the percentage of water in the brain of the albino rat from birth to 365 days. The four heavy dots represent the observation for man entered at equivalent ages. The graph for the percentage of water in the spinal cord is not discussed.

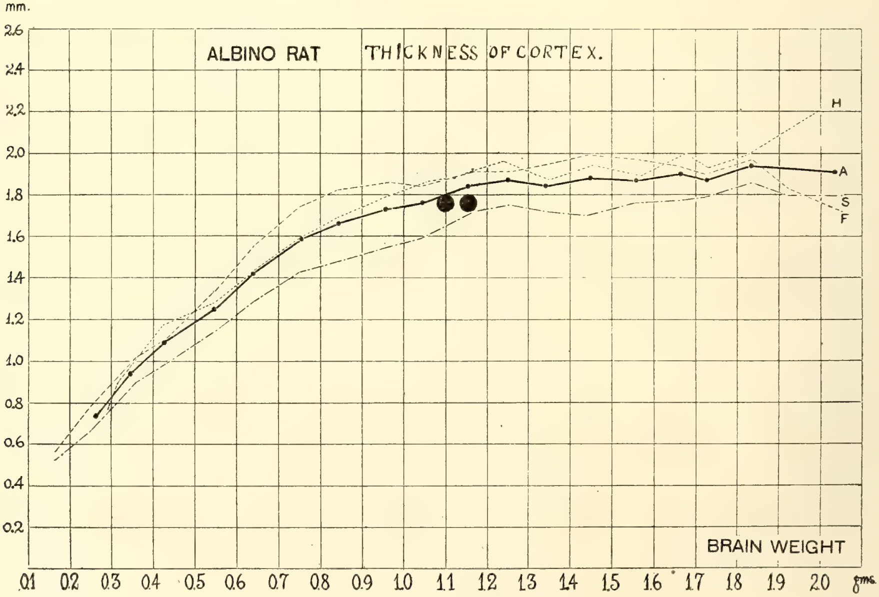

CHART 3

From Sugita '17, chart 9. Giving in millimeters the corrected thickness of the cerebral cortex of the albino rat; *H*, in the horizontal section; *S*, in the sagittal section; *F*, in the frontal section; *A*, the heavy line, represents the average of all of the measurements. The two heavy dots represent two incidental values for the human cortex—after reduction—entered at the equivalent ages.

The third instance is the maturing of the cerebellum, represented by the completion of the Purkinje cells and the disappearance of the external granule layer. In the rat these events occur between birth and 20 days of age, Addison '11. Like events occur in the human cerebellum and are completed in man at nearly the equivalent age. When the cerebellum has so far matured, locomotor control is attained in both forms, and thus this series of histological adjustments and locomotor control are accomplished at nearly equivalent ages in both the rat and man.

Finally, Dr. Sugita ('17) has just completed a study of the growth in thickness of the cerebral cortex of the rat, and the graph A in chart 3 shows that the mature thickness is nearly attained at the age of 20 days. There are at present no systematic studies on this point for man, but two incidental observations, entered as heavy dots, agree with the inference that at 15 months, the equivalent age, a like degree of completeness is reached by the human cerebral cortex, and therefore that only slight growth in the thickness of the human cortex is to be expected after this age.

There are therefore five prime events in the growth history of the nervous system of the rat, namely: (1) increase in total weight; (2) decrease in the percentage of water; (3) accumulation of myelin; (4) maturing of the cerebellum; (5) the attainment of the mature thickness of the cerebral cortex, all of which takes place at ages equivalent, or nearly equivalent, to those at which they occur in man.

It appears then that by the use of equivalent ages we have a satisfactory method for making a cross reference between the rat and man, and because the growth changes are similar in both forms, the rat may be used for further studies on the growth of the nervous system with the assurance that the results so obtained can be carried over to man.

Addison, William H. F., *Wistar Inst., Philadelphia, J. Comp. Neur.*, **21**, 1911 (459–481).
Donaldson, H. H., *Ibid.*, **26**, (1916), (443–451); these PROCEEDINGS, **2**, 1916, (350–356).
Sugita, Naoki, *J. Comp. Neur.*, **28**, 1917, (511–591).

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# VARIATION AND HEREDITY DURING THE VEGETATIVE REPRODUCTION OF ARCELLA DENTATA

By R. W. HEGNER

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The conclusions of several investigators, that the genotype is constant in organisms that are multiplying by fission, have recently been put in question by the work of Middleton[1] (1915) on *Stylonychia* and by Jennings[2] (1916) on *Difflugia*. Middleton obtained two lines of *Stylonychia* from a single specimen that differed constantly and markedly in their fission rate. Jennnings has shown that the descendants of a single specimen of *Difflugia* may be separated into a number of diverse lines that differ from one another in their heritable characteristics. The work herein described is part of an investigation that is being made of the species problem in the genus *Arcella* and the principal problem attacked is: Can heritably diverse lines with respect to spine number and diameter of shell be distinguished among the descendants of a single specimen of *Arcella dentata* produced by simple fission?

*Arcella dentata* (fig. 1) is a microscopic protozoon belonging to the lowest class, the Rhizopoda. It is as simple as any animal organism it is possible to obtain that has measurable characteristics. It varies in diameter from 73 microns to 150 microns and in spine number from 7 to 20. It multiplies vegetatively and rapidly and the characteristics of the shell are not modified by growth or by the environment, and are heritable but variable. In all, 6474 specimens were studied. Of these 171 were collected from a pond on the campus of the Johns Hopkins University at Homewood, Baltimore; 746 were reared from 70 of these specimens; and 5557 were obtained from the single specimen numbered 58. The number of generations represented by the progeny in family 58 was 69 and the average interval between generations was two and one-half days.

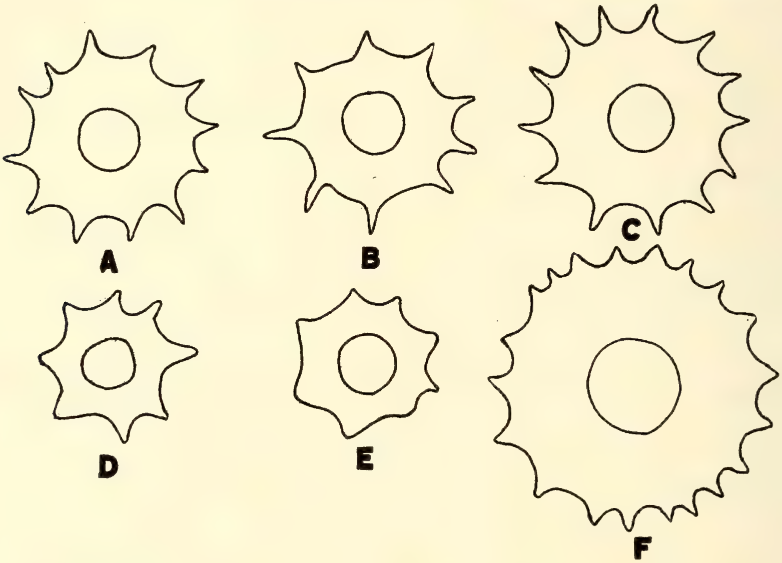

FIG. 1. OUTLINE DRAWINGS OF SPECIMENS OF *ARCELLA DENTATA* BELONGING TO FAMILY NO. 58. × 207

*A*, The progenitor of the entire family; *B*, a typical member of the low line *E*; *C*, a typical member of the high line *A*; *D*, the small progenitor of the line *EM*; *E*, a small specimen from the line *ED*; *F*, the largest specimen from the line *ED*.

One hundred 'wild' specimens were first selected at random from a large number taken from the pond. These varied in spine number from 7 to 13, and in diameter from 23 to 33 units (each unit being 4.3 microns). A marked correlation (0.325±0.060) was found between the spine number and diameter of these specimens.

Small families were then reared from 70 'wild' specimens selected so as to include large, small, and medium sized organisms. Seven hundred and forty-six specimens were obtained in this way, ranging in number from only 2 or 3 to 149 per family. The mean spine number of the families ranged from 10.40 to 14.07. Variations in spine number occurred among the descendants of single specimens during fission and these variations were in part inherited. It was found that the hereditary constitution of the different families was different with respect to spine number and the conclusion was reached that a 'wild' population consists of a large number of heritably diverse families so far as spine number is concerned, and also probably as regards diameter, since spine number and diameter are closely correlated.

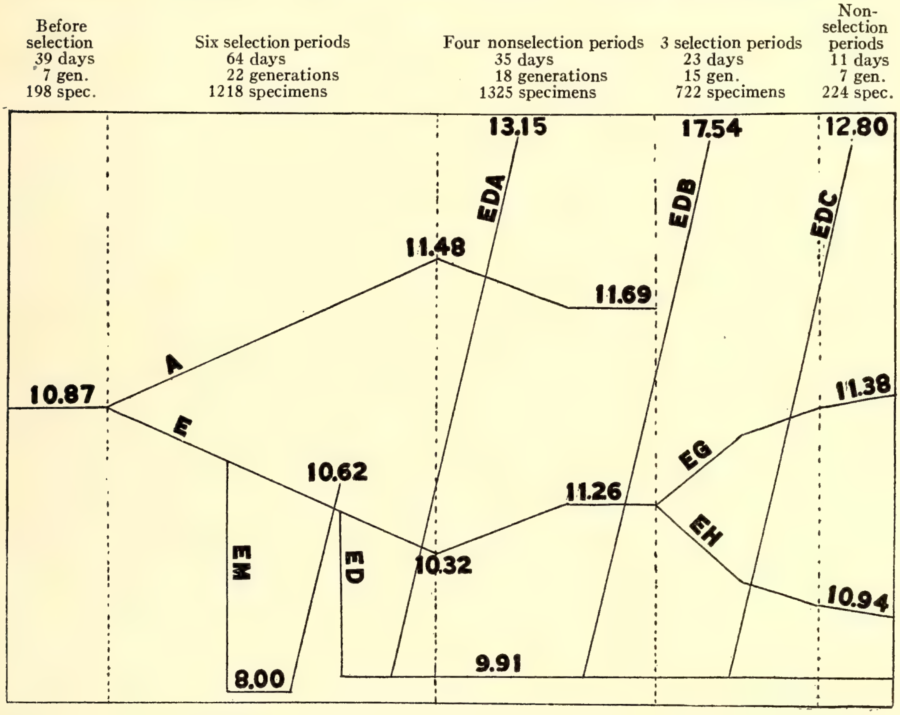

FIG. 2. DIAGRAM SHOWING THE MOST IMPORTANT HERITABLY DIVERSE LINES DERIVED FROM A SINGLE SPECIMEN OF *ARCELLA DENTATA* (NO. 58) BY FISSION

The character used was spine number. The letters indicate the designation of the lines, and the numbers are the mean spine numbers.

The main problem was next undertaken, i.e., an attempt was made to isolate heritably diverse lines from among the descendants of a single specimen produced by vegetative reproduction. A specimen, numbered 58 (fig. 1, *A*) was chosen for this work because it was near the mean of the species in diameter and spine number, and multiplied rapidly. Figure 2 shows the principal results of the experiments. During the thirty-nine days before selection was begun 198 specimens were obtained from number 58, representing 7 generations. These varied in spine number from 8 to 13, with a mean of 10.87. Selection was then inaugurated and carried on for six periods totalling sixty-four days. During this time 1192 specimens were reared, belonging to 22 generations. The work was divided into periods so that any changes due to environmental conditions would be revealed, and also because one investigator can take care of only a few hundred specimens at one time, and when the limit has been reached a new selection has to be made. At first all parents and progeny were kept until the end of each selection period, but later selection was also practiced during the periods. An effort was made to obtain a line (*A*) with a high mean spine number and another line (*E*) with a low mean spine number. Specimens within the high line that possessed 12 or more spines were selected and those within the low line with 10 or less. Past performance was used as the basis of selection, i.e., specimens in the high line that had a high spine number and had near relatives with a high spine number were chosen, and similarly specimens in the low line that had a low spine number and had near relatives with a low spine number were chosen for continuing the lines. During the six selections periods the differences between the mean spine numbers of the two lines were as follows: −0.07, 0.50, 0.40, 0.48, 0.84, and 1.16, and the mean difference was 0.55. The coefficients of correlation between parents and progeny with respect to spine number during these periods were 0.060 ± 0.076, 0.220 ± 0.039, 0.186 ± 0.042, 0.185 ± 0.040, 0.403 ± 0.044, and 0.512 ± 0.039.

Selection was then stopped and during four periods totalling thirty-five days, no selection was practiced. As many specimens as could be taken care of were kept during this time and as soon as one divided the 'parent' was eliminated and the offspring kept. In this way 1325 specimens were obtained belonging to 18 generations. At first regression occurred in both lines but later the mean difference between them remained almost constant. These differences for the four periods were 0.94, 0.07, 0.41, and 0.43, and the mean difference was 0.46. The decrease in the difference after selection was stopped was probably due to the production and inclusion within the high line of low spined specimens that would have been eliminated during the selection periods and within the low line of high spined specimens that likewise would have been eliminated. Each line, however, should give rise to as many high as low spined specimens, and hence the means of each would not vary after an equilibrium had been reached and the difference between them would be permanent. The coefficients of correlation between parents and progeny within the high line during these four periods was 0.170 ± 0.027 and within the low line 0.197 ± 0.024.

The low line (*E*) was then subjected to selection in a similar way and a high line (*EG*) and a low line (*EH*) were obtained during three selection periods of twenty-three days during which 722 specimens were produced belonging to 15 generations. The average difference between the mean spine numbers of the two lines during these three selection periods was 0.30. These selection periods were then followed by a nonselection period of 11 days during which 224 specimens were obtained belonging to 7 generations. The difference between the means of the two lines during this nonselection period was 0.44. It was concluded from these studies that two lines heritably diverse with respect to spine number, had been isolated from among the descendants of the low line (*E*).

Measurements were made during part of the nonselection periods to determine whether or not the diversities in spine number in lines *A* and *E* were accompanied by similar diversities in diameter. One hundred and twenty-seven specimens from the high line gave a mean diameter of 27.26 units of 4.3 microns each and 136 from the low line a mean diameter of 26.92 units. The difference of 0.34 unit shows that the two lines were different in diameter as well as in spine number, and that on the average the greater the diameter the more numerous are the spines. A marked correlation was found between the diameters of the parents and those of their progeny, the coefficient of correlation being 0.489 ± 0.035. A high correlation also existed between spine number and diameter, the coefficient of correlation being 0.255 ± 0.042.

Measurements were also made of the diameters of 384 of the progeny of the two branches (*EG* and *EH*) of the low line (*E*). These showed a mean difference in diameter of 0.26 unit corresponding to the difference in spine number.

These data prove that the descendants of a single specimen of *Arcella dentata* produced by vegetative reproduction differ slightly from one another in their hereditary constitution (fig. 1, *B* and *C*) and that heritably diverse lines may be isolated from among them, differing both in spine number and in diameter, and that these two characters are closely correlated. These heritably diverse lines resemble certain of the families that were reared from 'wild' specimens, and suggest that differences in the hereditary constitution of these wild specimens may have originated in the same way.

During the course of this investigation several branches were studied that arose from what seemed to be "mutations." These are indicated in figure 2 by the lines *EM*, *ED*, *EDA*, *EDB*, and *EDC*. These all appeared in the low line. Specimen *EM* had 8 spines and was only 18 units in diameter (fig. 1, *D*). Its parent had 10 spines and was 27 units in diameter. A large number of descendants (403) were reared from this small specimen, but it was found that the small diameter and lesser number of spines did not persist, but that the progeny of the fourth generation had regained the diameter and spine number of the low line from which *EM* was derived. The origin of this small specimen was therefore probably due to environmental conditions. Studies of this and certain other small specimens seem to show that it takes three or four generations for the progeny of a very small specimen to regain the normal diameter and spine number of the parental line.

The line derived from specimen *ED* is of special interest, since within it appeared the greatest diversities that were found during the entire investigation. The progeny of *ED* had a mean spine number of 9.91 and a mean diameter of 23.51 units (fig. 1, *E*). At the same time the mean spine number of the parent line (*E*) was 10.99 and the mean diameter was 27.05 units, giving a difference in mean spine number of 1.08 and in mean diameter of 3.54 units. Furthermore the differences persisted for many generations and until the line was discontinued. Specimen *ED* therefore fulfilled the conditions usually required of a mutation, i.e., it was a sudden large variation that was inherited.

From line *ED* there were derived three branches, *EDA*, *EDB* and *EDC*, that quickly exceeded in diameter and spine number any other branches in the entire family 58. The largest specimen appeared in branches *EDB*. It had 20 spines and a diameter of 40 units (fig. 1, *F*). These branches, however, soon died out for some unknown reason, although they were cultivated as carefully as possible.

The general conclusion reached is that within a large family of *Arcella dentata* produced by vegetative reproduction from a single specimen, there are many heritably diverse branches. These diversities are due both to very slight variations and to sudden large variations ('mutations'). The formation of such hereditarily diverse branches appears to be a true case of evolution that has been observed in the laboratory and that occurs in a similar way in nature.

[1] Middleton, A. R. J. Exp. Zool., **19**, 1915. (451–503.)

[2] Jennings, H. S. Genetics, **1**, 1916, (407–534.)

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# *THE IMPORTANCE OF NIVATION AS AN EROSIVE FACTOR, AND OF SOIL FLOW AS A TRANSPORTING AGENCY, IN NORTHERN GREENLAND*

By W. Elmer Ekblaw

Crocker Land Expedition, American Museum of Natural History, and University of Illinois

Communicated by J. M. Clarke, July 12, 1918

Nivation and solifluction, two closely related and important physiographic processes of Arctic lands, are perhaps nowhere better illustrated than in those coastal areas of northern Greenland not covered by the permanent ice-cap. The climate and the topography are favorable to the high development of these processes; the rather heavy snowfall that melts gradually during the short summer promotes the work of nivation; and the high relief, with numerous small plateaus and generally steep slopes, affords opportunity for the action of solifluction. The presence of an 'ice-table' everywhere, not deep below the surface, is an added favorable condition. As a consequence nivation and solifluction attain a degree of importance in northern Greenland not generally appreciated.

Nivation is the process by which quiescent neve effects the disintegration of rocks, and the destruction of some land forms, and the formation of others. In this process the snow itself produces very little, if any, effect; it is the water from the gradual melting of the snow that does the work. The melting must be so slow and gradual that the water does not escape by surface run-off, but soaks into the layer of rock fragments or soil above the ice-table, and then seeps slowly downward and outward from its source.

Nivation is assuredly most effective in regions of pronounced relief, and strong and variable winds, conditions prevalent in Greenland. Under such conditions, the snow does not lie as a mantle of uniform thickness, but is piled up in drifts of various kinds. For the sake of clearness, I have classified these drifts into two, or three, rather distinct divisions, more or less characteristic of the kinds of localities in which they are found. These divisions are (1) *dome-shaped drifts*, formed on the more or less uneven tops of plateaus, on the small plain areas, and on other relatively level surfaces; (2) *piedmont drifts*, formed along the foot of extended cliffs, or series of cliffs; and (3) *wedge drifts*, formed in gullies and small gorges near the top of cliffs.

The *dome-shaped* drifts vary in size with the size of the plateau or plain surface upon which they lie, and with the strength and character of the winds that blow over. On the small segments of a plateau surface dissected by gullies or stream beds, these dome-shaped drifts may not be very large or very thick; on larger segments, they may form one large dome, or several, all, or some, very thick,—in cases even becoming small ice-caps. Thus there may be every gradation from small dome-shaped drift to the great ice-cap of Greenland. If the winds that blow over vary considerably and rather uniformly in direction, the domes are somewhat symmetrical; if not, they slope gradually toward the windward side, and quite abruptly on the other. On the plateaus north of Foulke Fjord, the winds blow from almost all points of the compass, as often, almost, from one direction as another, and the drift slopes almost uniformly from the center to the whole peripheral edge; on Herbert Island, where the winds blow generally from the South, the dome slopes gently in that direction, while on the north side, the slope is abruptly precipitous. Very few of the tops of the plateaus are free from dome-shaped drifts; and many terraced moraines, deltas, and plains are covered by the same form.

The *piedmont* drifts are those that form along the cliffs on the windward sides of valleys, fjords, and straits, on the lee sides of capes, peninsulas, and islands. They are formed from the snow that comes drifting over the lands back of the cliffs, and piles up on the talus slopes below, at the foot of the cliffs. Thus, both sides of Foulke Fjord are bordered for almost nine months of the year by these piedmont drifts, those on the north side from the snow carried over by northerly winds, those on the south from the snow carried over by southerly winds. In Inglefield Gulf, only the south side is thus bordered because the cliffs on that side are more continuous, and the winds carry the snow from the plateaus over in great quantities; whereas on the north side, more broken by rather large valleys, the winds are deflected down the valleys, and little, if any, snow is carried over. Similar differences occur in other fjords.

The *wedge* drifts which form in gullies and small gorges near the top of the cliffs are genetically related to the piedmont cliffs, and consequently are distributed in much the same manner. They, however, are included within limited areas, and though in places they may be considered continuous with, and part of, the piedmont drifts, in other places they are the only drifts formed, for all the snow is carried into the gorges and gullies by the winds which are deflected into them The character of the drifts is so closely a function of the topography, the direction of the wind, and the consistency of the show, that as any one of these factors changes in character, the drifts may also change In Grenville Bay, for instance, the south side is bordered by piedmont and wedge drifts, while the north side is almost bare of snow; and since the south side gradually changes its direction from northeast-southwest at its mouth, to a nearly due east-west direction toward its head, and the prevailing wind is a general south-by-westerly, a regular succession develops, from tiny wedge drifts near the mouth, to a full-fledged glacier at the head, a glacier heading in a cirque a mile from the coast.

As long a quiescent neve covers the ground, and protects it from changes in temperature and from weathering, little disintegration or degradation can take place. It is only when the snow melts that the work begins. The melting in North Greenland is not a rapid process. Even when the sun shines at its highest, the temperature of the air does not rise much above 55° F.; it is usually lower, though that of exposed soil and rocks may be considerably higher. The snow drifts melt rather slowly, fastest at their edges.

Each of the kinds of drifts described produces different effects when melting. The dome-shaped drifts on the level surfaces melt first at their peripheral edges. The water formed is very near 32° F., and freezes at every drop of temperature due to cloudiness or change of wind so that excessive frost action takes place at the margin and just beyond, with consequent breaking up and disintegration of the rock. Often, too, the water freezes on the side of the drift away from the low sun, even at noondays, thus increasing the freezing action. Just beyond the margin of the drift, the temperature of the water is a little higher, and solifluction sets in. The disintegration of the rock, and the movement of resulting material, progresses in toward the center of the snow-drift covered area, as the drift melts back. Horizontal solifluction and consequent altiplanation terraces so clearly defined by H. M. Eakin,[1] are the immediate effect, and these in time result in reducing the plaeau top, or other plain surface, to a quite level surface, constantly being extended in area, and lowered. The process has been so well described by

Mr. Eakin that I merely call attention to this phase dependent upon the melting of the dome-shaped drifts.

The piedmont drifts formed on the lee slopes, or at the foot of the lee cliffs, act somewhat differently. The melting edges are at the top of the drift, and at the bottom. At the top the process is a sapping one, cutting back the cliff; at the bottom solifluction is the dominant process, though generally there is also some direct transportation by excess surface water that does not seep through the soil. The drift melts down from the top, and back from the foot. F. E. Mathes,[2] in his discussion of the glacial sculpture of the Bighorn Mountains, illustrates a cross section of such a drift, and the direction of the erosive attack upon the cliff. From the lower edge of the cliff where solifluction begins, the movement of the soil may continue to the foot of the slope, in one continuous sheet, or it may progress in a series of steplike slopes, or sloping terraces, with crescentic terminal margins, like festoons. Throughout northern Greenland these piedmont drifts are numerous, and almost invariably they give rise to similar solifluction slopes. When they occur on both sides of a V-shaped valley they tend to grade the sides and build up the bottom until it becomes U-shaped, as Mathes has described.

The wedge drifts formed in gullies and small gorges near the tops of cliffs, while acting in the same way as the piedmont drifts, produce different results. When these wedge drifts melt, the sapping process mentioned in the piedmont drifts cuts back the sides and the head of the gully or gorge in such a way as gradually to give it the form of a segment of a circle, the depth and extent of the segment depending upon the amount of snow blown into it. In this way a typical cirque may be initiated. When the snowdrifts become so large that they do not melt during the summer, ice gradually forms and a glacier occupies the floor of the cirque; frequently, though, the snow all melts away during the summer and no ice is formed, yet the cirque-form continues and the process goes on. A cirque in which ice has played no part can usually be distinguished by its rough and uneven floor, not at all like the scoured floor of a cirque once containing a glacier. The *bergshrund* in these high latitudes does not play an important part in cirque formationas it apparently does farther south, even in those cirques in which glaciers are formed; in the cirques carved by nivation alone there is, of course, no bergshrund at all.

Nivation is unquestionably of prime importance in the development of some of the topographic forms of the Greenland coast, and plays no small part in the degradation of the high cliffs, and the grading of the slopes.

Solifluction as defined by J. G. Andersson[3] is the process by which masses of the regolith saturated with water (which may come from melting snow or rain), flow slowly from higher to lower ground. This saturated, semi-fluid substance, not at all assorted as to size of fragments, moves along in much the same way as a glacier. H. M. Eakin[1] defined the process of solifluction as the migration of detritus under the thrust and heave of frost action. He recognized and described several types of soil movement and resultant topographic forms.

In northwest Greenland, solifluction is closely connected with nivation. For it is under conditions best suited to nivation that soil flow is best developed as a transporting agency. If precipitation be in the form of rain, most of the water flows away as surface runoff and its chief work is then the characteristic ordinary stream action. But if precipitation be in the form of snow—as it is in northern Greenland—which melts rather slowly and gradually, little of the water flows away on the surface. Most of it seeps into the ground, saturates it, and forms a more or less pasty mass according to the relative proportions of soil and water. The presence of an ice-table, in that it effectually prevents the seepage of water below the depth of the ice, facilitates soil flow. Thus, perhaps, the principal conditions necessary to solifluction are snowfall, with gradual melting of the snow, and an ice-table to prevent, or at least retard, the seepage of water deep into the ground.

Several forms of solifluction occur in Greenland, including probably all that have been observed elsewhere. Distinction should be made between solifluction which causes progressive motion of surface material such as results in altiplanation terraces, solifluction slopes, and soil streams or soil glaciers; and that which causes only circulatory movement such as may result in 'polygon-boden.' It is the first of these forms of solifluction which is one of the most important important transporting agencies in northern Greenland, and which has produced there land forms similar to those described by Eakin in Alaska, and by Andersson and others elsewhere. The other of these forms is also generally prevalent in Greenland, but while it is an active agent of movement contributory to the breaking up and degradation of the detritus, it is not so important as a transporting agent.

Throughout northern Greenland, every land area free of ice and snow during the short summer, exhibits the solifluction slopes and altiplanation terraces described by Eakin from Alaska. Both on the slopes and on the plateaus, the terraces resulting from solifluction are every where conspicuous. In northwestern Greenland, particularly, solifluction of these types is a most important transporting agency; the removal of detritus resulting from nivation, freezing and insolation, to the few torrential streams that bear it onward to the sea, is quite dependent upon solifluction. In many valleys the streams at the bottom of the valleys are not nearly large enough to remove the detritus brought down by solifluction, and the valley fast fills up, with lakes in the depressions, behind the dams of more abundant, or faster moving, detritus. On the gentler slopes, the rate of movement is not high, but on some of the steeper slopes the movement is rapid.

Though the type of solifluction resulting in solifluction slopes and altiplantation terraces is the dominant and most important type in northern Greenland, other types are very well represented. From this important type to rock slide on the one hand, and to circulatory movement that results in 'polygon-boden' on the other, every gradation of type of soil-flow may be found, and the combined results of their activities is a transportation of material as important as that of the streams and glaciers.

All the field evidence tends to show that nivation and solifluction, characteristic processes of disintegration and denudation under subarctic or arctic conditions, are of prime importance in the reduction of the high relief of northern Greenland.

[1] Eakin, H. M., *Washington, U. S. Geol. Survey, Bull.* 631, p. 76, 1916.

[2] Mathes, F. E., *Washington, U. S. Geol. Survey, 21st Ann. Rep.*, p. 181, 1899–1900.

[3] Andersson, J. G., *Chicago, J. Geol., Univ. Chicago,* **14,** 1906, p. 91.

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## ON THE α-HOLOMORPHISMS OF A GROUP

By G. A. Miller

Department of Mathematics, University of Illinois

Communicated by E. H. Moore, July 18, 1918

The term $\alpha$-holomorphism was introduced by J. W. Young to denote a simple isomorphism of a group $G$ with itself which is characterized by the fact that each operator of $G$ corresponds to its $\alpha^{\text{th}}$ power.[1] A necessary and sufficient condition that an abelian group of order $g$ admits an $\alpha$-holomorphism is that $\alpha$ is prime to $g$, and J. W. Young proved in the article to which we referred that when a non-abelian group admits an $\alpha$-holomorphism the $(\alpha-1)^{th}$ power of each of its operators is invariant under the group and the group admits also an $(\alpha - 1)$-isomorphism. Moreover, these conditions are sufficient for the existence of an $\alpha$-holomorphism.

The object of the present note is to furnish a complete answer to the following question: For what values of $\alpha$ is it possible to construct non-abelian groups which admit separately an $\alpha$-holomorphism? It will be proved that no such group is possible when $\alpha$ is either 2 or 3, but that for every other positive integral value of $\alpha$ there is an infinite system of non-abelian groups each of which admits an $\alpha$--holomorphism.

The fact that every group which admits a 2-holomorphism is abelian results directly from a theorem noted in the first paragraph of this article. If a non-abelian group $G$ admits a 3-holomorphism we may represent two of its non-commutative operators by $s_1$, $s_2$, and note that as a result of this holomorphism the two dependent equations

$$s_1^3 s_2^3 = (s_1 s_2)^3, \qquad s_1^2 s_2^2 = (s_2 s_1)^2$$

must be satisfied. Since $s_1^2$ and $s_2^2$ are invariant under $G$ it results directly from the latter equation that $s_1 s_2 = s_2 s_1$, and hence the assumption that $s_1$ and $s_2$ are non-commutative led to a contradiction. That is, *if a group admits a 3-holomorphism it must be abelian.*

We shall now prove that when $p$ is any odd prime number it is possible to construct a non-abelian group whose order is of the form $p^m$ and which admits a $(1 + kp)$-holomorphism, $k$ being an abitrary positive integer. Suppose that $k$ is divisible by $p^{\beta-1}$ but not by $p^\beta$. Hence $1 + kp = 1 + hp^\beta$, where $h$ is prime to $p$. Let $t$ be an operator of order $p$ which transforms an operator $s$ of order $p^{\beta+1}$ into its $(1 + hp^\beta)^{th}$ power. It will be proved that $t$ transforms each operator of the non-abelian group $G$ of order $p^{\beta+2}$ which is generated by $s$ and $t$ into its $(1 + hp^\beta)^{th}$ power.

This proof is an almost direct consequence of the two dependent equations[2]

$$(st)^p = s^p, \quad (st)^{hp^\beta} = s^{hp^\beta}$$

In fact, from these equations it results that $t$ transforms $s^{\alpha_1}t^{\beta_1}$ and $s^{\alpha_1}$ into the same powers. If we form the direct product of the group $G$ just constructed and any abelian group of type (1,1,1, . . . ) we clearly obtain another non-abelian group which is such that $t$ transforms each of its operators into a $(1 + kp)$-holomorphism. The group $G$ can therefore be used to construct an infinite system of groups each of which admits such a holomorphism.

To prove the theorem under consideration it is desirable to note that it is possible to construct a non-abelian group whose order is of the form $2^m$ and which admits a $(1 + 2^\gamma)$-holomorphism whenever $\gamma > 2$. In fact, if $s$ is an operator of order $2^{\gamma+1}$ and if $t$ is an operator of order 2 which transforms $s$ into its $(1 + 2^\gamma)^{th}$ power the non-abelian group of order $2^{\gamma+2}$ which is generated by $s$ and $t$ will clearly satisfy the required condition. Moreover, each one of the infinite system of groups obtained by forming the direct product of the group just constructed and an abelian group of order $2^l$ and of type (1,1,1,. . .) must likewise satisfy this condition.

It is now easy to establish the general theorem noted in the second paragraph. To construct a non-abelian group which admits an $\alpha$-holomorphism, $\alpha > 3$, it is only necessary to consider the factors of $\alpha - 1$. When $\alpha - 1$ is of the form $2^m$ any one of the groups described in the preceding paragraph satisfies the condition in question. When $\alpha - 1$ is not of this form let $p$ be any one of its odd prime divisors and suppose that $\alpha - 1$ is divisible by $p^\beta$ but not by $p^{\beta+1}$. Hence $\alpha$ is of the form $1 + hp^\beta$ considered above and it has been proved that *whenever* $\alpha > 3$ *it is possible to construct an infinite system of groups such that each group of this system admits an* $\alpha$*-holomorphism.*

---

[1] J. W. Young, *New York; Trans. Amer. Math. Soc.*, **3**, 1902, (186).

[2] Miller, Blichfeldt and Dickson, *Theory and Applications of Finite Groups*, Wiley, New York, 1916, p. 108.

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# MEASURING THE MENTAL STRENGTH OF AN ARMY

By Major Robert M. Yerkes
Sanitary Corps, N. A.

Communicated by R. Pearl. Read before the Academy, April 23, 1918

Committees of the American Psychological Association and the Psychology Committee of the National Research Council prepared between April and August, 1917, various psychological methods for the selection and classification of recruits in the Army and Navy. In August, 1917, the Surgeon General on recommendation of Majors Victor C. Vaughan and William H. Welch of the National Research Council accepted for trial certain methods which had been devised especially for the psychological examination of men enlisted in the United States Army. Dr. Robert M. Yerkes, Chairman of the Psychology Committee of the National Research Council was appointed on August 17, Major in the Sanitary Corps, to organize and direct this new branch of the service for the Medical Department.

During the initial development of this service, Major Yerkes worked under the immediate administrative supervision of Major Pearce Bailey, Chief of the Division of Neurology, Psychiatry and Psychology. Plans were speedily prepared and the necessary authorization secured from the Secretary of War for thorough trial of the proposed methods in four National Army cantonments. For this work sixteen psychologists were commissioned first lieutenants in the Sanitary Corps, and twenty-four others were given civil appointment by special authorization of the Secretary of War. The psychological staff of each camp consisted of ten men.

Between October 1 and December 1, 1917, nearly 100,000 drafted men, students in Officers' Training Camps, and officers of camps or divisions were examined. In December the work was officially inspected by an officer of the Medical Corps. The reports of this inspectional officer supplemented importantly the statistical data supplied during progress of work by Major Yerkes. As the results of psychological examining indicated clearly the value of the work for the elimination of men mentally defective, the balancing of organisations in mental strength, the classification of men for the assistance of personnel officers in the camps, and the indication of men of exceptional intelligence to be charged with special responsibility or sent to Officers' Training Camps, and as all reports of the inspector were favorable, the Surgeon General in December 1917 recommended to the Secretary of War the continuance and the extension of psychological examining to the entire army. This recommendation, after careful inquiry concerning the value of the work by the Training Committee of the War College Division of the General Staff, was approved by the Secretary of War who directed the Surgeon General to prepare a plan for the proper conduct of the proposed work.

A comprehensive plan was promptly prepared by the Staff of the Section of Psychology, Division of Neurology, Psychiatry and Psychology. This plan provided for suitably trained personnel, special psychology building in each camp, and necessary apparatus and printed materials. It was fully approved by the Secretary of War, Jan. 19, 1918, and the Surgeon General was authorized to create a Division of Psychology in his office, and to put the plan of psychological examining into effect.

A school for military psychology was immediately organized in connection with the Medical Officers' Training Camp, Fort Oglethorpe, Georgia, in which a maximum of 100 student officers and approximately 100 psychologists enlisted in the Medical Corps could be trained simultaneously during a period of two months. This school was opened February 4 with Captain William S. Foster, San. C., N.A. as senior instructor. Between this date and July 1, 1918, approximately 70 officers of the Sanitary Corps and 250 men enlisted in the Medical Corps were given special training in military psychology.

Plans for suitable psychology buildings were prepared and duly submitted to the Quartermaster General. Subsequently the Equipment Committee of the General Staff disapproved the expenditure for these buildings pending Congressional appropriation for the purpose, and by special request of the Secretary of War, existing buildings were assigned for psychological examining in most of the divisional training camps.

The necessary apparatus and printed materials for the examining of 500,000 soldiers were speedily prepared, and during April and May, psychological examining staffs were organized in twenty-five National Army and National Guard camps. During the month of June additional apparatus and printed materials were manufactured for the examining of 1,000,000 soldiers.

On July 1, 1918, psychological examining was in progress in twenty-eight army camps and in three General Hospitals. Seventy-nine officers of the Sanitary Corps were on duty in these stations, and in the Division of Psychology, Surgeon General's Office. Approximately 100 non-commissioned officers and privates of the Medical Corps especially trained in military psychology had been assigned to duty in examining stations, and somewhat more than 100 were in training at the school of military psychology, Fort Oglethorpe. Up to April 27, 1918, when new methods were put into use, approximately 140,000 soldiers had been examined. On July 1, 1918, 600,000 had been examined. Of this number, slightly more than 0.25% had been recommended by psychological examiners to psychiatrists for discharge because of mental deficiency, and about 0.5% had been recommended for assignment to service organizations for development battalions because of mental inferiority.

Psychological examinations which were originally conducted in wards of Base Hospitals, are now made in a psychology building located usually in the Depot Brigade. In this building, drafted men are examined promptly on reporting to camp. Their intelligence ratings are immediately transmitted to the Personnel Officer of the camp. All cases of mental deficiency, or those for which psychiatric examination is indicated as desirable, are referred to the psychiatrist.

The aim of the Division of Psychology, Surgeon General's Office, is to develop a psychological center in each military training camp to which all psychological problems of military assignment, training, discipline, morale, and intelligence may be referred by officers of the line or staff. Such a center exists in twenty-five camps, and the service is being extended as rapidly as available personnel permits.

(Publication approved by the Board of Publications, Office of the Surgeon General.)

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# THERMO-ELECTRIC ACTION WITH THERMAL EFFUSION IN METALS: A CORRECTION

By Edwin H. Hall

Jefferson Physical Laboratory, Harvard University

Communicated August 3, 1918

The last paragraph of my paper in these Proceedings for April, 1918, dealt incorrectly with the effect of 'hypothesis (B)' the hypothesis that free electrons in the interatomic spaces of an unequally heated metal bar have a tendency like that which in ordinary gases produces the phenomena of thermal effusion.

In a common gas the condition of equilibrium maintained by thermal effusion is not $p$ constant but $p \propto T^{\frac{1}{2}}$, where $T$ is the absolute temperature. In dealing with the free electrons, for which the $R$ of the equation $pv = NRT$ may not be a constant, we have as the condition which thermal effusion tends to create

$$p \propto (RT)^{\frac{1}{2}} \tag{1}$$

whence

$$dp = \frac{p}{2}\left(\frac{dR}{R} + \frac{dT}{T}\right). \tag{2}$$

If we take one gram of electrons, each of mass $m$, we have

$$pv = \frac{1}{m} RT, \tag{3}$$

and by substitution in (2) we get

$$dp, \text{ or } (dp)_s = \frac{1}{2v} \cdot d\ (pv). \tag{4}$$

This is the difference of pressure which the free electrons within a metal could bear without drift and without the help of electrical force. It may be called the *static difference of pressure*, $(dp)_s$.

If, now, there exists within a metal a free-electron pressure $dp > (dp)_s$, the excess

$$dp - (dp)_s, \text{ or } dp - \frac{1}{2v} \cdot d\ (pv), \tag{5}$$

will be the true mechanical driving force operating to maintain a drift of the electrons. Hence the introduction of 'hypothesis (B)' requires the substitution of $dp - (1 \div 2v)\ d(pv)$ for $dp$ in the second member of equation (4), of my paper referred to above, and makes the whole equation read

$$(P_c - P_h) + \int^c \frac{k_f}{k} \cdot dP_f + \int_h^c \frac{k_a}{k} \cdot dP_a = \frac{1}{Ge} \int_c^h \frac{k_f}{k} \left( vdp - \tfrac{1}{2} d\ (pv) \right), \tag{6}$$

where $k$ has the same meaning as the $(k_a + k_f)$ used in the original equation.

This is the expression for the 'virtual e.m.f.' resident in a detached metal bar having one end at $T_h$ and the other at $T_c$. In my previous discussion of this matter, not written out in my April paper, I had proceeded as if the factor $(k_f \div k)$ could be put with the $(pv)$ so as to give $d\,[(k_f \div k)(pv)]$. This error led to my giving figure 4 of that paper as the graphical representation, on the $P$-$V$ plane, of the virtual e.m.f. in question.

The second member of equation (6) can be put into the form

$$\frac{1}{Ge} \int_c^h \frac{k_f}{k} \left( \tfrac{1}{2}\, vdp - \tfrac{1}{2}\, pdv \right),$$

and the graphical representation of this expression is given in figure 1. Here $A\ D$ is the $pv$ line for one gram of free electrons in the metal; $A'\ D'$ is obtained from $A\ D$ by applying the proper value of the factor $(k_f \div k)$ to each value of $v$; and $A'\ D'$ is obtained for $A\ D$ by applying the proper value of the factor $(k_f \div k)$ to each value of $p$. $A''\ D''$ is obtained from $A'\ D'$ by taking one-half of each value of $v$, so that the area $E\ A''\ D''\ G$ represents the part $\frac{1}{2} \int_c^h \frac{k_f}{k} \cdot vdp$ of (7). $A_1''\ D_1''$ is obtained from $A_1'\ D_1'$ by taking one-half of each value of $p$, so that $A_1''\ D_1''\ d\ a$ represents the part $-\frac{1}{2} \int_c^h \frac{k_f}{k} \cdot pdv$ of (7).

The sum of these two areas, multiplied by $(1 \div Ge)$, represents the virtual e.m.f. in question.

If the line $A\ D$ were of constant *pv*, as it might be if we were dealing with an isothermal 'alloy bridge,' the two shaded areas would be equal and their sum would equal $E\,A'\,D'\,G$. That is, there would be no tendency to 'thermal effusion' in such a case, and the virtual e.m.f. would reduce to

$$\frac{1}{Ge}\int_c^h \frac{k_b}{k}\cdot v\,dp,$$

the value it has under hypothesis (A).

The total effective e.m.f. for a closed circuit made up of two different metals and two isothermal alloy bridges is represented by $(1 \div Ge)$ times the sum of two areas like $A''\,B''\,C''\,D''$ and $A_1''\,B_1''\,C_1''\,D_1''$ in figure 2, where the path $A\ B\ C\ D$ represents the *pv* changes of one gm. of *free* electrons throughout the circuit.

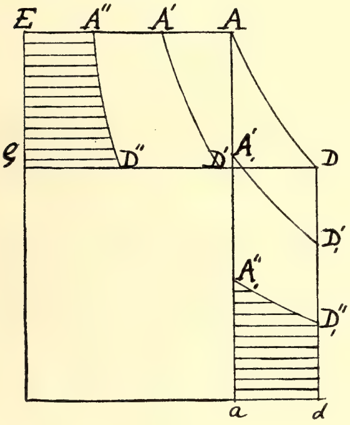

FIG. 1

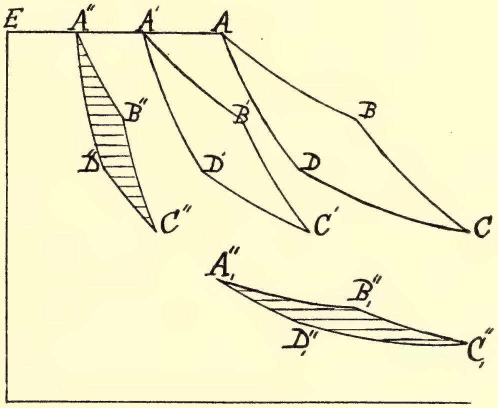

FIG. 2

The total effective e.m.f. of the circuit is not, as I have previously believed it to be, necessarily the same under hypothesis (B) as under hypothesis (A). For the latter case it is represented in figure 2 by $(1 \div Ge)$ times the area $A'\,B'\,C'\,D'$, the boundary of which corresponds to the changes of pressure and volume undergone by 1 gram of electrons, in part free electrons and in part associated electrons, in going around the circuit as part of a current.

I had overlooked the interesting fact that, when we have to do with thermal effusion, which overrides the tendency to equality of pressure throughout a gaseous body, we can no longer calculate the work done by or on a body of gas by means of a mere diagram of its pressure-volume changes. Thus, in a vertical cylinder containing a column of air, kept warmer at the top than at the bottom, a porous partition extending partly across the cylinder would constantly transmit air upward through its interstices, to be returned downward past the edge of the partition, and work could thus be done by means of air acting in a *pv* cycle of no area.

# INVARIANTS AND CANONICAL FORMS

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Every student of the theory of invariants has observed the fact that the coefficients of a unique canonical form are invariants. But a general *a priori* proof of this fact, sufficiently general to cover all of the cases needed in the applications, seems to be lacking. It is the purpose of this paper to furnish such a proof, making use only of the abstract principles which are common to all known invariant theories.

We begin by giving a brief outline of some of these invariant theories, so that we may have these instances in mind when we formulate our general theory. Consider first a binary $n$-ic,

$$(p) = \sum_{i=0}^{n} p_i x_1^i x_2^{n-i}. \tag{A}$$

This binary *form* is a function of $p_0, p_1, \ldots, p_n$ and of $x_1, x_2$. The $p$'s are called the *coefficients*, and the $x$'s the *variables* of the form. We introduce new variables by putting

$$\bar{x}_i = \alpha_{i1} x_1 + \alpha_{i2} x_2 \quad ,(i = 1, 2),$$

where the quantities $\alpha_{ik}$ are arbitrary constants with a non-vanishing determinant, thus transforming the form $(p)$ into a new form $(\bar{p})$. Those combinations of the coefficients $p_k$ which are equal to the same functions of the coefficients $\bar{p}_k$ are called *invariants* of the form. In the classical theory of invariants it is really the *equation* $(p) = 0$ which is the object of study rather than the *form* or *function* $(p)$. Consequently the additional transformations, operating upon the coefficients only, $\bar{p}_i = \lambda p_i$, $(i = 0, 1, 2, \ldots, n)$, where $\lambda$ is an arbitrary constant, are introduced. An invariant of the *equation* must remain unaltered by these transformations also.

A second invariant theory is concerned with the class of linear differential expressions or forms

$$p_0 \frac{d^n y}{dx^n} + p_1 \frac{d^{n-1} y}{dx^{n-1}} + \ldots . + p_n y, \tag{B}$$

where $p_0, p_1 \ldots, p_n$, and $y$ are functions of $x$. The *coefficients* $p_k$ in this case are functions of $x$, whereas in instance (A) they are numbers, real or complex. In instance (A) the variables $x_1, x_2$ are also numbers; in (B) the variables $y, dy/dx, \ldots, d^n y/dx^n$ are functions. In this case we may even think of $y$ as the *only* variable, since the variables $dy/dx$, etc., are determined when $y$ is given.

Let us now transform the variable $y$ by putting $y = \lambda(x)\bar{y}$, where $\lambda(x)$ is an arbitrary function. Then (B) goes over into a new differential form $(\bar{B})$

whose coefficients $\bar{p}_k$ depend upon $p_0, p_1, \ldots, p_n$ and $\lambda$. The corresponding invariants are of considerable importance. Here also we must distinguish between the theory of invariants of forms, and the theory of invariants of equations.

As a third instance, let us consider the class of analytic functions

$$f(x) = p_0 + p_1 x + p_2 x^2 + \ldots + p_n x^n + \ldots, \tag{C}$$

where the coefficients $p_0, p_1 \ldots, p_n, \ldots$, and the variable $x$ are complex numbers. We introduce transformations of the form

$$\bar{x} = \frac{\alpha x + \beta}{\gamma x + \delta}, \quad \alpha\delta - \beta\gamma \neq 0,$$

where $\alpha$, $\beta$, $\gamma$, $\delta$ are arbitrary constants, and attempt to find invariants of $f(x)$ under all such transformations. In this case we are dealing with a form which has infinitely many coefficients, forming a denumerable set.

We shall list one further instance of our general theory. Let $K(x, \xi)$ be a real continuous function of $x$ and $\xi$ in the region $0 \leqq x \leqq 1$, $0 \leqq \xi \leqq 1$, and let $\varphi(\xi)$ be a real continuous function of $\xi$ in the interval $0 \leqq \xi \leqq 1$. Then

$$I(x) = \int_0^1 K(x, \xi)\, \varphi(\xi)\, d\xi \tag{D}$$

may be regarded as a form whose value, as function of $x$, depends upon the choice of the $K$ and $\varphi$ functions. We may think of the functions of $x$ to which $K(x, \xi)$ reduces, for all special values of $\xi$ between 0 and 1, as the coefficients of the form. Thus the number of these coefficients is non-denumerable, even if the function $K(x, \xi)$ be regarded as given. As to the variables of the form $I(x)$ we still have two choices. We may think of the *function* $\varphi(\xi)$ as ranging over the class of all continuous functions and regard $\varphi(\xi)$ as the only variable of the form; or else we may consider the *functional values* of $\varphi(\xi)$ as the variables. In the latter case, the *range* of the variables would be the class of real numbers, and the number of variables would be continuously infinite.

We may transform (D) by putting

$$\varphi(\xi) = \lambda(\xi)\, \bar{\varphi}(\xi),$$

where $\lambda(\xi)$ is an arbitrary continuous function of $\xi$. This will transform $I(x)$ into $\bar{I}(x)$, where

$$\bar{I}(x) = \int_0^1 \bar{K}(x, \xi)\, \bar{\varphi}(\xi)\, d\xi, \quad \bar{K}(x, \xi) = K(x, \xi)\, \lambda(\xi).$$

In particular it will be possible to choose $\lambda(\xi)$ in such a way as to make

$$K(\tfrac{1}{2}, \xi) = 1$$

except for those values of $\xi$ for which $K(\frac{1}{2}, \xi) = 0$; the resulting integral may be said to be in its *canonical form*.

For the purposes of our general theory, which shall include all of the instances mentioned as well as infinitely many others, *we postulate a class* [*F*] *of forms, or functions of two general arguments;* $F(p, x)$. As the notation indicates, the arguments of such a form are of two kinds, the *coefficients* $p$, and the *variables* $x$. Both $p$ and $x$ are supposed to be *general variables* in the sense of E. H. Moore, each argument varying on its own range. This range may be a continuous or discrete class of numbers, or a class of functions, or any other well-defined range; it may be the same for $p$ and $x$, or different Finally the range over which $F$ varies may be different from either or both of the ranges of $p$ or $x$. In the language of ordinary analysis this general formulation includes single forms or systems of forms, whose coefficients and variables may be finite or infinite in number, and in the latter case denumerably or non-denumerably infinite.

We further assume that *the class of forms* [*F*] *is well-defined.* This means that a criterion is given by means of which we may decide whether a given form does or does not belong to [$F$]. This criterion will also enable us to distinguish between the coefficients and the variables of the form

*Two forms of the class* [*F*] *are said to be identical if their corresponding coefficients are equal.* Equality of the correspondng variables is *not* required for the identity of two forms. For this reason it is frequently convenient to suppress the notation for the variables in the symbol for a form, and to use the simpler symbol $F = (p)$ to replace $F(p, x)$.

*We postulate, in the second place, a group of transformations, which operates upon the variables of a form* $F$ *of* [$F$] *and transforms every* $F$ *of* [$F$] *into another form* $\bar{F}$ *of the same class. The coefficients* $\bar{p}$ *of* $\bar{F}$ *will depend on the coefficients* $p$ *of* $F$ *and upon elements which occur in the transformation used. We assume that we obtain in this way a new group* $G$ *operating upon the coefficients of the form.* $G$ *is said to be induced by* $g$.

In this postulate the word group is uded in the usual sense. Thus we call a set of operations a group if the identity is included among its operations, if the operations possess an associative law of combination, if the product of any two operations of the set and also the inverse of every operation of the set belong to the set. The two groups, $g$ and $G$, may however contain a finite number of operations or infinitely many; in the latter case they may be discrete or continuous; if they are continuous, they may be finite or infinite, in the sense of Lie.

*By an appropriate modification of the group* $G$, *we may include in our theory not only the invariants of forms, but also the theory of invariants of equations and systems of equations.* This has been pointed out already in connection with our preliminary discussion of instance (A).

*If there exists at least one transformation in the group* $G$ *which, when applied to a form* $F_1$ *of* [$F$], *transforms* $F_1$ *into a form* $F_2$, *the two forms* $F_1$ *ad* $F_2$ *are said to be equivalent under the group* $G$.

Let us now define a proper sub-class $[\Phi]$, of $[F]$, by imposing some property upon the coefficients $p$ which is not satisfied by all forms of $[F]$. It is assumed that this property is well defined, so that we may be able to decide whether a given form $F$, of $[F]$, does or does not belong to the sub-class $[\Phi]$. Let us assume that, for every form $F$ of $[F]$, there exists in $G$ at least one transformation $S$ which transforms $F$ into an equivalent form of the sub-class $[\Phi]$. *We shall then say that* $\Phi = S[F]$ *is a canonical form of* $F$. It may happen that such a canonical form equivalent to $F$ under the group $G$, and belonging to the sub-class $[\Phi]$ exists merely for those forms of $F$ which do not belong to a well-defined sub-class $[\Psi]$ of $[F]$. We shall speak of the forms of the sub-class $[\Psi]$ as *exceptional* forms, and call all the other forms of $[F]$ *generic* forms. Of course the term, *general* form of $[F]$, includes both the exceptional and the generic forms. Under these circumstances we shall still speak of $\Phi$ as a canonical form of $F$, but we shall add the qualifying phrase *for the generic case* whenever the distinction becomes necessary.

In general there will be many transformations of $G$ which transform every generic $F$ into a canonical form of the sub-class $[\Phi]$. If all of these transformations transform every generic $F$ into the same form $\Phi$, of the sub-class $[\Phi]$, we shall say that *the canonical form* $\Phi$ *is unique.*

It remains to define the term *invariant.* A function $I$, of the coefficients $p$ of a form $F$, is an absolute invariant under the broup $G$, if it is equal to the same function of the corresponding coefficients of any form $\overline{F}$ which is equivalent to $F$, by means of a transformation of the group $G$.

This notion may be regarded as including also the notion *covariant.* For, we may replace the given form $F$ by another form, or system of forms, who coefficients depend also upon the *variables* of $F$.

We are now ready to prove the following theorem.

*Let* $[F]$ *be a well-defined class of forms, and let* $[\Phi]$ *be a proper sub-class of* $[F]$. *Let G be a group of transformations which transforms every form of* $[F]$ *into a form of the same class. By a generic form of* $[F]$ *we mean one which is equivalent to a form of* $[\Phi]$ *under G. Then, there exists, for every generic form of* $[F]$ *at least one transformation in G which transforms F into a canonical form of the sub-class* $[\Phi]$. *If this canonical form is unique, its coefficients are one-valued absolute invariants of the form F for the group G.*

*Proof.*—Let $F$ be a generic form of $[F]$, and let $\Phi$ be its canonical form. Let $S$ be the most general operation of $G$ which transforms $F$ into $\Phi$. We shall have symbolically

$$S(F) = \Phi. \tag{1}$$

The operation $S$ will depend, in general, upon the coefficients $p$, of $F$, and may contain, besides, arbitrary elements in great number. The canonical form $\Phi$ belongs to the sub-class $[\Phi]$ of $[F]$. Since this canonical form is, by hypothesis, unique, its coefficients $\pi$ are independent of the arbitrary elements which may occur in $S$, and they are one-valued functions of the coefficients of $F$ in the sense, that when $F$ is given, the coefficients of $\Phi$ are determined uniquely. We may express this symbolically by the equation

$$\pi = U(p), \tag{2}$$

where $U(p)$ is the symbol for a one-valued function of the coefficients $p$.

Now let $T$ be any transformation of $G$, and let

$$\bar{F} = T\,(\bar{F})$$

be any form of $[F]$ equivalent to $F$ under $G$. Since we assumed that $F$ was generic, and since

$$F = T^{-1}(F),$$

we conclude that $\bar{F}$ is also generic. In fact we find

$$S(F) = ST^{-1}\,(F) = \Phi,$$

so that $ST^{-1}$ is an operation of $G$ which transforms $\bar{F}$ into a canonical form of the sub-class $[\Phi]$.

Let $\bar{S}$ be the most general operation of $G$ which transforms $\bar{F}$ into a canonical form of the sub-class $[\Phi]$, so that

$$\bar{S}\,(\bar{F}) = \bar{\Phi},$$

and let $\bar{\pi}$ denote the coefficients of $\bar{\Phi}$. These coefficients will depend on the coefficients $\bar{p}$ of $\bar{F}$, in exactly the same way as the coefficients $\pi$ of $\Phi$ depend upon the coefficients $p$ of $F$. That is, we shall have, in a manner analogous to (2),

$$\bar{\pi} = U\,(\bar{p}). \tag{3}$$

We also know that

$$\bar{\Phi} = \Phi;$$

for we have seen already that $ST^{-1}$ will transform $\bar{F}$ into $\Phi$, and we are assuming that every generic form $\bar{F}$ of $[F]$ has a *unique* canonical form of the sub-class $[\Phi]$.

But, if two forms of a class are equal, their corresponding coefficients are equal. Therefore we have $\bar{\pi} = \pi$ or, according to (2) and (3).

$$U\,(\bar{p}) = U\,(p).$$

In other words the coefficients of $\Phi$ are indeed absolute invariants of $F$ under the group $G$, and therefore our theorem is demonstrated.

If the forms of the class $[F]$, which are generic forms from the point of view of the canonical form chosen, do not constitute the whole of $[F]$ there will remain in $[F]$ certain exceptional forms constituting a sub-class $[H]$ of $[F]$. But the theorem may be applied to these exceptional forms as well, whenever a unique canonical form exists for a generic one of the exceptional forms; but of course, the canonical form in this case will not belong to the class $[\Phi]$, but to a certain sub-class $[X]$, of $[H]$. If the forms of $[H]$ are not all generic from this new point of view, we may adopt a new canonical form for the exceptional ones, and continue in this way.

We have observed that the coefficients of a unique canonical form are absolute invariants, and moreover one-valued invariants, in the sense, that their values are uniquely determined as soon as the coefficients of the form $F$ are given. In the ordinary theory of algebraic invariants, it is at once apparent that these invariants are algebraic functions of the coefficients of $F$. Consequently it follows from their one-valuedness that, in this case, these invariants are *rational* functions of the coefficients. In the theory of invariants of linear differential equations, the uniquenes of a canonical form gives rise to invariants which are rational functions of the coefficients of the differential equation and of their derivatives.

There are many cases in which a $k$-valued canonical form is obtained rather than a unique one. That is, if we resume our terminology, the sub-class $[\Phi]$ contains not merely one, but exactly $k$ forms $\Phi_1, \Phi_2, \ldots, \Phi_k$, each of which is equivalent to $F$ by a transformation of the group $G$. *The coefficients of these canonical forms will still be absolute invariants of $F$, but they will be $k$-valued functions of its coefficients.* It is obviously possible to find an equation of degree $k$ with one-valued invariants as coefficients, of which these $k$-valued invariants are the roots. In the theory of algebraic invariants we obtain in this way irrational invariants, as roots of an equation whose coefficients are rational invariants.

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# TYPES OF PHOSPHORESCENCE

By Edward L. Nichols and H. L. Howes

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Communicated, August 1, 1918

The existence of phosphorescence of exceedingly short duration was long ago revealed by the phosphoroscope of Becquerel but, until very recently the afterglow of luminescent bodies has been studied quantitatively, only where it is of comparatively long duration. Curves of decay were supposed to be all of the same character. It was assumed that the law of diminution of brightness, as expressed by the equation

$$I = \frac{1}{(a_1 + b_1 t)^2} + \frac{1}{(a_2 + b_2 t)^2}$$

was of general application and that the phosphorescence of various substances differed only in color, brightness and duration.

The measurements of Waggoner[1] and of Zeller[2] on phosphorescence of short duration tended to confirm this view. On the other hand the observations of

Ives and Luckiesh[3] on the effect of temperature upon the curves of decay of certain phosphorescent sulphides might seem to demand a modification of the usual law in certain cases, while the studies of the phosphorescence of gases by C. C. Trowbridge[4] and of paraffine at liquid air temperatures by Kennard[5] indicate that for the range covered by their measurements the law of decay cannot be expressed by a summation of $1/(a + bt)^2$ terms.

In a recent investigation[6] we have found the exceedingly brief phosphorescence of the uranyl salts, which, although very brilliant lasts only for about 0.03 seconds, to be of an entirely different type. It decays very slowly at first and later very rapidly, whereas in all cases previously studied the opposite is true. Measurements just completed on the luminescence of calcite indicate that this newly determined form of decay curve is not confined to the uranyl salts.

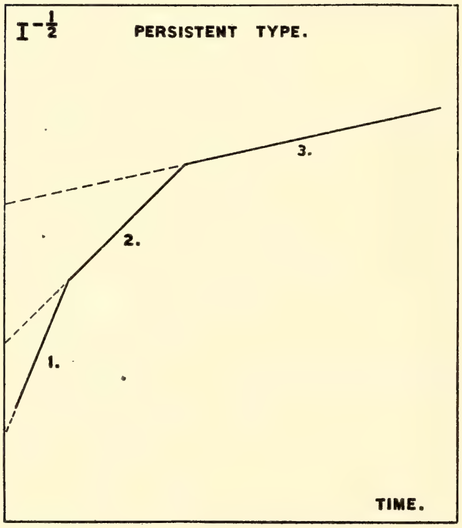

FIG. 1

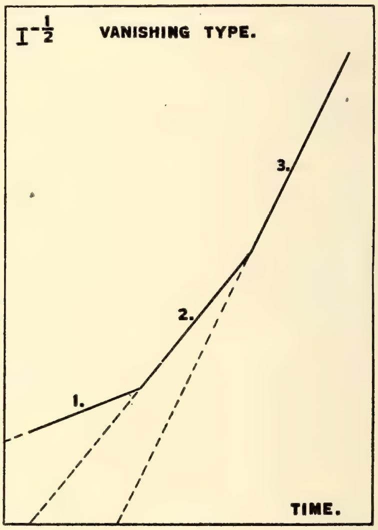

FIG. 2

We propose therefore to recognize two distinct types of phosphorescence and to designate them as *persistent phosphorescence* and *vanishing phosphorescence.* They are sharply distinguished from one another by the following characteristics.

*Types of phosphorescence.—1:* (Characteristic of persistent phosphorescence.) This type has a curve of decay made up of a succession of linear process, of diminishing slope as we proceed from the origin of time (fig. 1). Three or more such processes have been found in all cases which have been studied through a sufficient range.

*2:* (Characteristic of vanishing phosphorescence.) This type has a curve of decay made up of a succession of linear processes, of increasing slope as we proceed from the origin of time (fig. 2).

It is understood that in these specifications and elsewhere, unless otherwise stated, the curve of decay is plotted with time from the close of excitation as abscissae and the reciprocal of the square root of intensity ($I^{-\frac{1}{2}}$) as ordinates. A linear process is any straight portion of the graph; as *1*, *2* or *3* in figures 1 and 2. The inference is that the more or less abrupt changes of slope are related to and indicative of real changes in the *processes* by which the emission of light from the phosphorescing body is being maintained.

A distinguishing criterion of type is found in the sign of the intercepts of the various processes at the origin of time. In type 1 the intercepts are all positive; in type 2 the intercepts of process 2 and 3 are negative.

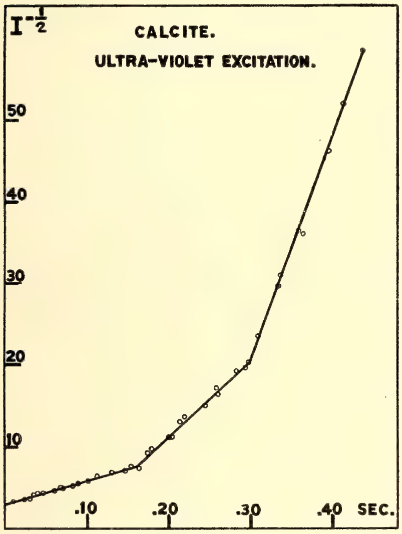

FIG. 3

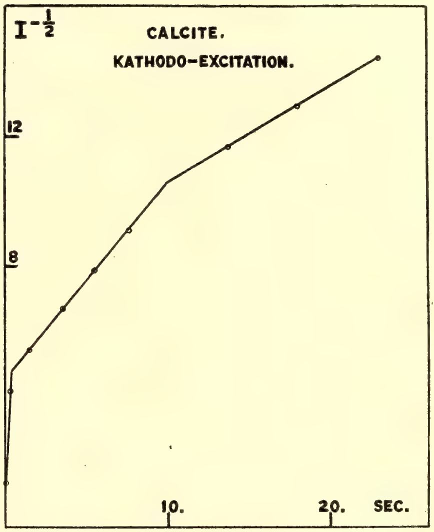

FIG. 4

The passage from one process to the next is presumably never actually discontinuous, as in the above diagrams, but it is sometimes very abrupt as in the curves of figures 3 and 4 which are from our recent measurements of the phosphorescence of calcite. Sometimes the transition from slope to slope is very gradual; so that the first and second processes tend to merge into a curved line. We have shown,[6] in the case of the uranyl salts that the location of the knees depends on the intensity of the excitation and that distributed knees—so to speak—occur when the exciting light penetrates the crystal and being absorbed produces luminescence of decreasing intensity in successive layers; also that where the conditions are such that excitation is confined chiefly to the surface, the knees are well defined.

Similar modifications of the curve occur in kathodo-excitation, and distributed knees are then probably due to a kathodic discharge containing particles of varying velocity.

*Both types may occur in a single substance.*—Shortly after the completion of our experiments on the vanishing phosphorescence of the uranyl compounds, Misses Wick and McDowell[7] found that some of the same salts, notably $K.UO_2(NO_3)_3$ and $K_2UO_2(NO_3)_4$ when exposed to the kathode discharge at the temperature of liquid air glowed for many seconds after the close of excitation with decay curves of the persistent type. At + 20° the effect is either absent or excessively feeble. Not all the uranyl salts, even among those that are reasonably stable in vacuo, respond to the action of the kathode discharge to a measureable extent.

The Franklin Furnace calcites, as we discovered in our recent investigation,[8] have the same remarkable property, i.e., vanishing phosphorescence under photo-excitation and persistent phosphorescence under the kathode discharge. Figures 3 and 4 exhibit the vastly different behavior of the same calcite after the close of these two modes of excitation.

Both types of phosphorescence are obtainable with calcite at any temperature between $-180°$ and $+300°$.

*Independence of the two types.*—Since the kathode discharge modifies the surface layers of substances subjected to its action, it might be supposed that calcite crystals after kathodo-bombardment would show persistent phosphorescence when photo-excited. To test this supposition we placed a crystal of the Franklin Furnace calcite in the bottom of a V shaped vacuum tube having a quartz window.

It could thus be excited either by the kathode discharge or from an iron spark. Previous excitation by the kathode rays had no observable effect on the photo-phosphorescence which was of the vanishing type and had approximately at least the normal color, brightness and duration. The photo-phosphorescence could be superimposed upon the persistent kathodo-luminescence, as a fleeting effect, at any time during the life of the latter either after or before the close of the kathodo-excitation. Apparently the two were entirely independent of each other.

Misses Wick and McDowell had previously made a similar observation in their study of the kathodo-phosphorescence of the uranyl salts; i.e. that the kathode rays do not render them capable of persistent photo-phosphorescence.

*Apparent occurrence of both types with a single source of excitation.*—Willemite is one of the most brlliant of luminescent substances. The afterglow is commonly fleeting, but masses are occasionally found which exhibit phosphorescence of long duration. Such specimens are persistent under photo-excitation and kathodo-excitation alike. The phosphorescence of the other variety, unlike that of calcite and of the uranyl salts, is of short duration under both kinds of excitation.

The determination of the decay of phosphorescence of a specimen of the latter variety, using the disk phosphoroscope, gave curves like that plotted in figure 5. Processes 1 and 2 are of type 1 (vanishing) but these are followed by a process 3 of lesser slope.

This is obviously a composite curve due to the superposition of phosphorescence of both types. It does not follow however that it is a true example of a *single substance* brought to both types of phosphorescence by photo-excitation. The evidence is to the contrary.

1. The two varieties of willemite are commonly associated. One of our specimens contains parallel veins of the persistent form and of willemite of short duration in the same matrix.

2. Willemite is associated with the Franklin Furnace calcite.

That the vanishing phosphorescence in the curve in figure 5 may be due to an admixture of calcite is suggested by the following observations.

*a.* When the disk of the phosphoroscope, coated with the powdered willemite and exposed to the light of the iron spark, was driven rapidly, the region nearest the spark in the direction of revolution but shielded from the direct light was a brilliant yellow-green, the regions approaching the spark but shielded, were of a dark *blue*-green. This is the effect which would be expected were calcite present, since its red-yellow phosphorescence would give the yellowish caste during the first process of decay but would vanish before the revolution of the disk was completed.

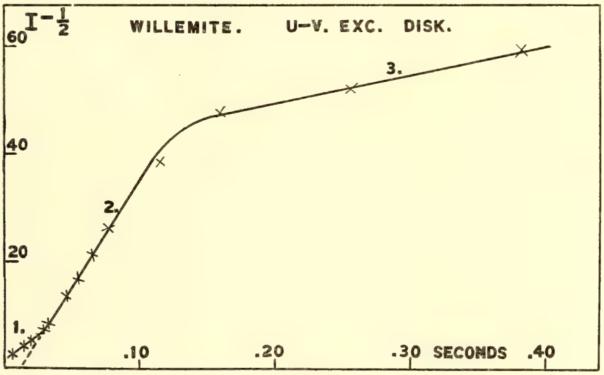

FIG. 5

*b.* When the collimator of a spectroscope was directed to the rapidly revolving disk the spectrum of the phosphorescent willemite appeared as a broad continuous band extending from the extreme red to the blue. The crest was approximately at 0.54 $\mu$ and there was suspicion of a weak region in the yellow.

At a somewhat lower speed, which allowed a longer interval of time between excitation and observation the red end of the spectrum disappeared and at a much lower speed the yellow and brightest part of the green vanished leaving a much narrower band in what had been a comparatively feeble region, with its crest in the blue green at about 0.52 $\mu$.

The former brilliant crest at 0.54 $\mu$ was now very dim and lay near the edge of the persistent band. This persistent crest coincides in position with that of the fluorescence spectrum of a willemite of long duration determined spectrophotometrically some years ago.[9]

Obviously we have in this spectrum two, and perhaps three overlapping bands. A very rapid red band, presumably due to calcite, the band of the willemite of short duration and a persistent band of small intensity indicative of the presence of willemite of long duration.

Not all specimens of willemite have the complicated curve of decay above described. Waggoner,[1] (1908) in his studies of phosphorescence of short duration, determined the beginnings of the curves of decay of two willemites; one of the rapid and one of the persistent variety. He used the Merritt phosphoroscope having a range up to 0.06 seconds. His curves are of the persistent type.

*Not all phosphorescence of quick decay is of the vanishing type.*—That the type of phosphorescence is not altogether a question of duration we have abundant evidence. The willemite of quick decay measured by Waggoner is of much shorter measurable duration than the Franklin Furnace calcites, yet its curve of decay is clearly of the opposite type. The same is true of various phosphorescent compounds prepared by Waggoner, i.e., $ZnCl_2$, CdCl, $CdSO_4$,—each with a trace of $MnSO_4$ added, which were heated to redness with a flux of $Na_2SO_4$. Although their phosphorescence is of brief duration they gave decay curves of the persistent type.

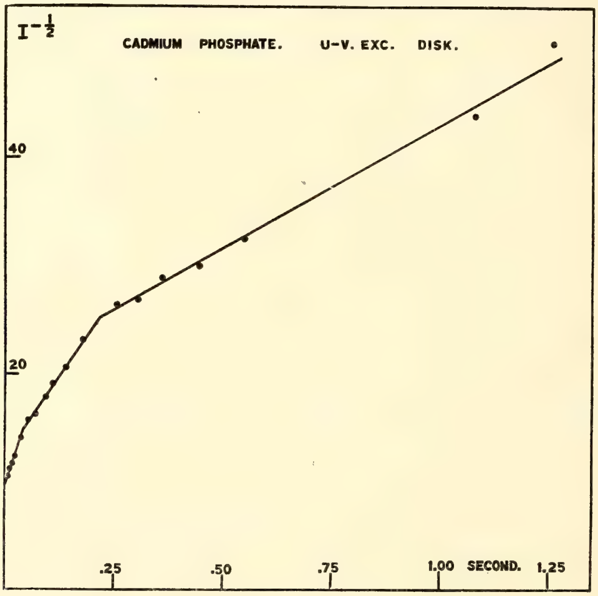

FIG. 6

There is this distinction between the luminescence of such substance and vanishing phosphorescence. The latter becomes completely extinct almost immediately after it ceases to be of measurable intensity; as indicated by the steepness of the second and third processes (fig. 2). In the persistent type of short duration the loss of light during the initial process may go to the very limit of visibility within a few hundredths of a second or less, but if the threshold is not actually crossed the phosphorescence may remain, barely visible in a completely dark room, for a considerable time. Waggoner remarks (on page 216 of his paper) "*It will be noted that in practically all the substances studied, the measurable portion of the decay is over in 0.07 seconds. Some of the substances may, however, be seen in a dark room for a very much longer time.*"

Phosphorescence of short duration, when *not* of the vanishing type, may be considered simply as a case of the persistent type in which the rapid initial process reduces the intensity nearly to or beyond the threshold of visibility. The subsequent processes, though gradual are therefore too faint for observation or invisible. The brightness of the first process may be as great or greater in cases of quick decay than where the phosphorescence continues of measurable intensity for a long time. Zeller[2] who has studied the beginnings of decay of the phosphorescence of various persistent compounds has made note of the fact that duration is not in any simple way related to initial brightness. In a group of phosphorescent cadmium compounds, in particular, some that were too dim for measurement with the phosphoroscope remained visible for a long time while those of greatest initial intensity were of very short duration.

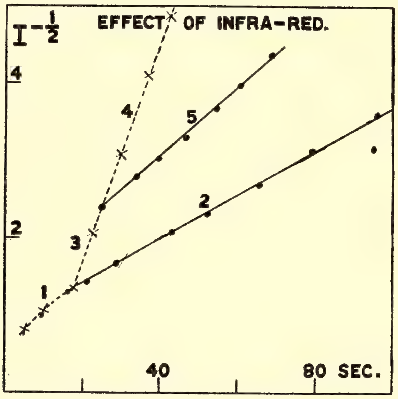

FIG. 7

An interesting substance which came to our notice during these investigations through the kindness of Mr. W. L. Lemcke is a cadmium phosphate prepared by Mr. W. S. Andrews. Under the iron spark it is excited to a fine white phosphorescence having a measurable duration of about one second. We determined its curve of decay with the disk phosphoroscope. As may be seen from figure 6 it is of the persistent type and so far as the white afterglow is concerned might be classed as of rather short duration; but a very faint ruddy phosphorescence remains for a much longer time. This specimen, as viewed on the rotating disk is remarkable for the succession of color effects which it exhibits. Pink, very fleeting, is followed by nearly pure white, then for an instant by blue which goes over into a very persistent pink, turning ruddy as decay progresses.

Such color changes in other substances have been shown to be due to the existence in the phosphorescence spectrum of overlapping bands having different rates of decay and the striking display would seem to indicate an unusual complexity of composition. We were surprised, therefore, to learn from Mr. Andrews that for the production of this brilliant white phosphorescence cadmium phosphate of exceptional purity is necessary, and that there is no admixture of other metallic salts, as in the preparation of the phosphorescent sulphides.

Curves exhibiting the new vanishing type of decay, above described, resemble nothing in the earlier work on phosphorescence so much as the curves obtained as a result of the action of infra-red light on Sidot Blende.[1] Figure 7 exhibits this phenomenon. Processes 1 and 2 together show the pure decay curve, without infra-red, but if the infra-red is applied after 18 seconds of decay the more rapid process 3–4 ensues. If, however, the infra-red is removed at the end of 25 seconds the decay resumes a slower rate, as shown by process 5. Processes 1, 3 and 4 taken together show a striking resemblance to the vanishing type of decay. Attempts to hasten the decay of calcite or of the phosphorescent uranyl salts by infra-red action however have yielded no result. Here the successively steeper processes must be due to a change in the rate of recombination of the ejected electrons with the active phosphorescing groups. It is commonly believed that only certain groups of particles are active, the majority being considered to be inactive. It is evident that these changes in rate of decay are inherent with the crystal and must be due to a more or less sudden change in the internal conditions. The electric fields within a crystal are so strong that it is perhaps not surprising that the infra-red field, applied from the outside, cannot affect either the phosphorescent action in the active groups or increase the number of free electrons. The sudden change in the rate of decay may be due to a change in the electric field concomitant with the changed orientation of the charged particles, which drives the ejected electrons in greater proportion to the non-active parts of the molecule.

[1] Waggoner, *Physic. Rev., Ithaca,* (Ser. 1), **27**, 1908, (209).

[2] Zeller, *Ibid.*, **31**, 1910, (367).

[3] Ives and Luckiesh, *Astroph. J., Chicago,* **36**, 1912, (330).

[4] Trowbridge, C. C., *Physic. Rev.*, **32**, 1911, (129).

[6] Kennard, *Ibid.*, (Ser. 2), **4**, 1914, (278).

[6] Nichols and Howes, *Ibid.*, **9**, 1917, (292).

[7] Wick and McDowell, *Ibid.*, **11**, 1918, (421).

[8] To be described in a forthcoming number of the *Physical Review.*

[9] Nichols and Merritt, *Ibid.*, (Ser. 1), **28**, 1909, (349).

[10] Nichols and Merritt, *Studies in Luminescence, Carnegie Inst., Washington, Pub..* 152, 1912.

# *THE SMITHSONIAN 'SOLAR CONSTANT' EXPEDITION TO CALAMA, CHILE*

By C. G. Abbot

Smithsonian Astrophysical Observatory

Communicated August 16, 1918

As early as 1903 the observations of the Smithsonian Astrophysical Observatory suggested the view that the solar radiation varies over a range of several per cent within intervals as short as a few days or weeks. We were measuring the radiation of the sun at the earth's surface. The measurements comprised determinations of the heating effect of the solar rays on the blackened surface of the pyrheliometer, which measures all rays of the spectrum as found in 'white light,' and also the heating effects at all parts of the solar spectrum as detected by the bolometer, including ultra-violet, visible and infra-red rays. We measured at intervals on every clear day as the sun declined from high altitudes near the zenith to low altitudes near the horizon. Thus the rays measured passed through longer and longer paths, according to the obliquity with which they crossed the atmospheric layers, and consequently they grew weaker and weaker as the sun declined lower and lower. From the spectro-bolometric measurements, standardized to calories by aid of the simultaneous pyrheliometer measurements and reduced to zero atmospheric absorption by the method of Langley we thus determined the intensity of solar radiation as it would be outside the atmosphere at mean solar distance. This has been called the solar constant of radiation. Its average value is about 1.93 calories per square centimeter per minute.

As I have said the results of 1903 at Washington indicated variations of this so-called constant over a range of nearly 10%. Owing to the prevalence of clouds at Washington all too few observations were available. Nevertheless when we compared such as we had with the anomalies of temperature of the North Temperate Zone as represented by meteorological observations at 89 stations, all regions showed a variation of temperature nearly parallel to, and of a proper magnitude to correspond with, the supposed variations of the sun.

In 1905 we transferred the observing to Mount Wilson, California, and with the exception of 1907 we have observed the 'solar constant,' in that relatively favorable place, usually from June to October of each year. The results have confirmed the apparent variability of the sun. It is impossible to go outside the atmosphere to observe, and we feared that the apparent variability of the sun might have been really due to defects in our estimation of the losses in the atmosphere. To check our work as far as possible we observed in 1908, 1909, and 1910 from the summit of Mount Whitney (4420 meters) the highest peak in the (older) United States. No error dependent on altitude appeared. In 1914 we sent an automatic recording pyrheliometer by sounding balloon to 25,000 meters altitude. The result obtained was trustworthy and agreed with what we expected. All these checks confirmed the accuracy of our work and strengthened our belief in the solar variability.

Meanwhile in 1911 and 1912 I had observed in Algeria while my colleagues observed in California. Unfortunately for the proof of solar variability 1911 was cloudy and 1912 was the year the air was charged with volcanic dust by the great volcanic eruption of Mount Katmai, June 6, 1912. Nevertheless despite this handicap the results left little reasonable doubt that the sun is variable. High 'solar constant' values at Bassour, Algeria, coincided with high values at Mount Wilson, California, and vice versa, and equal increments of radiation were found at the two stations independently, notwithstanding that they are separated by one-third the earth's circumference.

In 1913 and subsequently the proof of solar variability was rendered impregnable. We investigated daily the distribution of radiation over the solar image formed by our tower telescope on Mount Wilson. The sun's image is, as you know, unequally bright at centre and edges, so that the curve of its intensity along a diameter takes the form of the letter U inverted. The steepness of the curves of the U varies with wave-length. But we found also that it varies from day to day, and that the variations are such that a greater contrast of brightness between center and edge occurs when the solar radiation as a whole is found to be diminished and vice versa. I suppose the outer layers of the sun vary in transparency. When more opaque they diminish the 'solar constant' but as the effect is greatest near the limb of the sun where the oblique path of the ray in the solar layers is greatest, the result is also to increase the contrast. At all events we have found a true variation of the sun, independent of the earth's atmosphere, which coincides in time of its fluctuations with the observed changes of the 'solar constant.'

Professor Pickering had kindly undertaken pyrheliometer measurements at Arequipa, Peru. These were carried on from 1912 to 1917. They tended to confirm in the more outstanding features the variation of the sun observed at Mount Wilson.

Dr. L. A. Bauer, whose remarkable campaign of magnetic observations lends lustre to the science of our country, has investigated certain minute disturbances of terrestrial magnetism for which no cause had been assigned. He finds them to be closely correlated with the variations of solar radiation we have observed.

Dr. H. H. Clayton, formerly of Blue Hill Meteorological Observatory, now of the Argentine Meterological Service, has investigated the variations of terrestrial atmospheric temperature and pressure for nearly fifty stations in all parts of the earth. He finds a close correlation of these meteorological variations with the irregular variations we have observed in solar radiation. Equatorial stations show a direct correlation in temperature. That is high solar radiation is followed by high temperature and vice versa. Many temperate zone stations show opposition of variation. Polar stations show direction variation. If these results shall be confirmed and enlarged they bid fair to aid in actual weather forecasting, for the changes are by no means small.

In view of the scientific and utilitarian interest associated with the variability of the sun, I have long desired that several cloudless observing stations might take up 'solar constant' work. In 1914 I made a trip to Australia expecting that the Australian Government would take it up. This hope was frustrated by the war.

In 1916 Secretary Walcott appropriated from the income of the Hodgkins Fund to equip and maintain for several years such a station in South America, but owing to the war it was temporarily located in the North Carolina mountains in 1917. The station proved very cloudy, and now it has proved possible though very expensive to go to Chile.

After correspondence with the South African, Indian, Argentine and Chilean meteorological services I became convinced that near the nitrate desert of Chile is to be found the most cloudless region of the earth easily available. Dr. Walter Knoche of Santiago has most kindly furnished two years (1913 and 1914) of unpublished daily meteorological records for a number of Chilean stations. In his judgment and mine the best is Calama on the Loa River, Lat. S. 22° 28′, Long. W. 68° 56′, Altitude 2250 meters. For the two years the average number of wholly cloudless days is at 7 a.m. 228; 2 p.m., 206; 9 p.m., 299; and of wholly cloudy days, none. The precipitation is zero; wind seldom exceeds 3 on a scale of 12; temperature seldom falls below 0° or above 25°C.

Our expedition, Director Alfred F. Moore, Assistant Leonard H. Abbot, reached Calama June 25, 1918, equipped with a full spectro-bolometric, pyrheliometric, and meteorological outfit of apparatus, as well as with books, tools, household supplies, and everything which we could furnish to make the work successful and life bearable. We are under great obligations to the Chilean Government for facilitating the expedition in many ways, and to the Chile Exploration Company who have given the expedition quarters and observing station at an abandoned mine near Calama. Many others in Antofagasta, Chuquicamata and Calama have been of great assistance.

The apparatus is set up in an adobe building about 30 feet square, in which the observers also have sleeping apartments. A 15-inch two-mirror coelostat reflects the solar beam to the slit of the spectro-bolometer. We use a Jena ultra-violet crown glass prism and speculum metal mirrors in the spectroscope. The linear bolometer is in vacuum, and constructed in accord with complete theory for greatest efficiency. Its indications as measured by a highly sensitive galvanometer are recorded photographically on a moving plate which travels proportionally to the movement of the spectrum over the bolometer. Successive bolometric energy spectrum curves each occupying 8 minutes of time are taken from early morning till the sun is high and are thus recorded on the plate. Their intensity indications at 40 spectrum positions are reduced by aid of a special slide rule plotting machine.

A pair of silver disk pyrheliometers is read simultaneously with each spectro-bolographic determination. Measurements of humidity, temperature, and barometric pressure accompany the bolometric observations. Also a pyranometer is employed to determine the total sky radiation.

The young men find pleasant companions at the great copper mine at Chuquicamata. At present they are boarding with a Chilean family at Calama, but as both are good cooks they may wish to board themselves. The railway and the river both pass the town of Calama, so that there is no such desert isolation as might be feared. To the east are the Andes Mountains. The peaks in that neighborhood rise to 16,000 or 17,000 feet. Some are volcanic but none of these are very near.

We hope the work will be continued for several years at least, and that nearly daily observations may be obtained. The application of the results to meteorology is something which may prove to have great possibilities. To exploit them we must first possess a long and nearly unbroken series of solar radiation observations.

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## *MAROON—A RECURRENT MUTATION IN DROSOPHILA*[1]

By Calvin B. Bridges

Marine Biological Laboratory, Woods Hole

Communicated August 23, 1918

The recessive eye-color 'maroon' was one of the early mutations in Drosophila (found March 13, 1912). This eye-color was found in a stock culture of wild flies, and was at first supposed to be a new appearance of the recessive mutation 'purple' discovered about a month earlier. Genetic tests (crosses between the two lines, etc.) showed that the new color was not the old purple, but was a new mutation almost identical with purple in appearance but entirely independent in origin and inheritance. This was the first of our now numerous cases of eye-color 'mimics,' in which two distinct genes produce practically the same somatic effect.

During the six months following the discovery of purple and maroon there were thirteen recorded appearances or 'purple' eye colors, which constituted our most striking 'mutating period' of 'epidemic of mutation.' Of these new 'purples,' tests showed that the first, fifth, sixth, and thirteenth were maroon, while the rest were true purple. A study of the pedigrees showed that these maroons had come from two independent occurrences of the maroon mutation, while the true purples likewise came from at least two separate acts of mutation. Maroon has reappeared independently on two subsequent occasions, so that this particular mutative process has occurred at least four times. The same property of specific recurrent mutation has since been found to be characteristic of several other loci, of which notch, vermilion, rudimentary, and cut are the most striking examples.

At this time (April, 1912) pink was the only mutation known to be in the third chromosome (by definition) so that the $F_2$ results from the cross of maroon to pink constituted the only test as to whether maroon also is in the third chromosome. Maroon differs enough from pink in its characteristics so that the separation of the two forms could be accurate for about 85% of the individuals. The $F_1$ flies from the cross were wild-type (two non-allelomorphic recessives), and the $F_2$ flies were 485 wild-type: 215 maroon-like: 183 pink-like: 0 maroon pink.

It was expected that the flies which were homozygous for both maroon and pink would be as much lighter than pink as maroon is lighter than the wild-type, since this was the usual type of relationship. No flies lighter than pink were found, but it was uncertain whether this meant that the double recessive was actually absent because of strong linkage (no crossing over), or that the double form was present but indistinguishable from pink or from maroon. That the double form should be present, but not lighter than pink, would mean that maroon and pink are 'non-modifiers' of each other. Subsequent work has shown that 'non-modification' is the usual relation between the different pinks that have arisen.

That the apparent absence of the maroon pink class was a real absence due to linkage was soon proved: Tests, by means of $F_2$ and back cross results, of the linkage relations between maroon and the second chromosome mutant "arc" showed that these two genes assort with complete independence. The above tests showed that the locus of maroon was probably not in the second chromosome, and was therefore probably in the third chromosome. Meanwhile the body color ebony had arisen and had been shown to be linked to pink (Sturtevant, '13).[2] Since there could be no question of failure of identification of the double recessive maroon ebony the next step was to show that maroon is linked to ebony. The $F_2$ of the cross of maroon by ebony gave a 2 : 1 : 1 : 0 linkage ratio, which we had just learned to explain as the result of 'no crossing over in the male' (Morgan, '12).[3] The $F_2$ from the maroon by pink cross was thus proved to have been a 2 : 1 : 1 : 0 ratio, and was in fact the first such linkage result obtained for the third chromosome.

The location within the third chromosome of the gene for maroon was made easy and direct by the discovery and location of two excellent III chromosome dominants, 'dichaete' and 'hairless.' The locus of dichaete is quite near the left end of the third chromosome (at about 11.0) while that of hairless is near the middle (at about 32.0). The back cross tests showed that the locus of maroon is to the right of dichaete by about 4.2 units, or is at a position of 15.2 (11.0 + 4.2) when referred to the locus of sepia as the zero point. With hairless, maroon gave 21.2% of crossing over, which corresponds to the location of maroon between dichaete and hairless.

It was found that the chromosome in which the third independent mutation to maroon had occurred was carrying two other mutant genes: One of these, 'dwarf,' is a recessive that reduces the size of flies to only about half that of normal sibs; it also sterilizes females homozygous for it. The locus of dwarf is within a unit to the right of that of maroon. The other gene causes no observable somatic difference, but profoundly affects the process of crossing over in third chromosome in definite regions and by definite amounts. A distinct new system of cross-over values corresponds to the heterozygous and still another to the homozygous condition for the gene. A comparison of the behavior and effects of this gene with that of the already known genetic varition called '$C_{III}$' showed that they were probably identical mutations. There was found to be 5.9% of crossing over between dichaete and maroon in females heterozygous for $C_{III}$, and 6.6% when females were homozygous for $C_{III}$. In the region near dichaete there is, as these values show, no great difference between the homozygous and heterozygous $C_{III}$ conditions.

[1] A full account of maroon with the detailed data on which the various statements of this paper are based will appear as a section of a publicatian by the Carnegie Institution of Washington.

[2] Sturtevant, *Science, New York*, N. S., **36**, 1913, (990–992).

[3] Morgan, *Ibid.*, **36**, 1912, (718–720).

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# *THE 'HOMING HABITS' OF THE PULMONATE MOLLUSK ONCHIDIUM*[1]

By Leslie B. Arey and W. J. Crozier

Bermuda Biological Station for Research, Dyer Island, Bermuda

Communicated by E. L. Mark, September 18, 1918

For some time it has been known that the limpets and their allies living on shore rocks exposed by the falling tide exhibit the habit of returning, on the approach of the period of low water, to particular spots where they have individually made 'homes,' slight depressions in the rock-surface, which usually fit with considerable exactness the form and irregularities of the limpet's shell. From this 'home' the limpet may creep away 50 to 90 cm., when covered by the sea, to feed—frequently following well-defined paths, to which it also adheres when creeping homeward again. Such experimental work as has been directed to the study of this peculiarity has gone to show that it may be regarded as depending upon tactile and perhaps other forms of irritability (Bohn, Piéron), the mollusk remembering (possibly) a sensory map of the topography of its home region. It cannot be said, however, that this matter is as yet thoroughly understood. 'Homing' behavior of a similar sort has also at various times been attributed, in an anecdotal way, to different terrestrial slugs and snails.

It seems previously to have escaped attention that homing movements of a more striking character are to be found in another gastropod, the intertidal naked pulmonate Onchidium. We have studied in this respect the behavior of Onchidium (Onchidella) floridanum Dall, and although further investigation of the subject has been planned, it is thought well to record here the nature of certain of our findings.[2]

The homing phenomenon to which we refer may be characterized as follows: Onchidium lives in groups, or communities, numbering up to a dozen or more individuals in each, which during high tide find shelter in cavities within the eroded shore rock; these cavities are either narrow crevices, or may be subspherical with a diameter as great as 6 cm.; but in any case they communicate with the exterior by means of a small and almost undetectable opening (usually single), commonly obstructed by the growth of small mussels (Modiolus). When, during daylight hours, the tide has fallen to such an extent that an Onchidium 'nest' has been above water-level for about half an hour, which frequently happens about two hours before extreme low tide (but, for many nests, does not occur at all during neap tides), the Onchidia in that nest creep out, in succession, wander some distance over the algae-covered rocks, on which they feed, and then (at least an hour before their nest would again be covered by the rising of the tide) all return to that nest. The period of exposure lasts about two hours. Any one community may include individuals of varying lengths up to 2 cm.

The Onchidia return each to its own particular nest, even on much-eroded rocks and where the nests are many and close together, and that in spite of the fact that the individuals emanating from different nests frequently mingle during their wanderings. The snails comprising anyone community begin the return to their nest almost simultaneously, although they may be scattered over an area a meter in diameter and may be situated on opposite sides of the nest.

Clearly, at least two distinct problems are presented by this behavior: First, why do the Onchidia return to the nest, and above all, why do the various members of a group begin almost simultaneously to make this return? Secondly, what is the nature of the directive control of the return journey itself? For the present, we discuss only the latter problem.

Onchidia situated at some distance from their nest pursue a fairly direct path toward it when they begin to return; previously, their paths may have been quite irregular. One or more approximately linear depressions may be adhered to in the immediate vicinity of the nest itself, but this is not necessary. An individual taken from one side of its nest, and placed on the opposite side, at a distance of a meter or slightly more, frequently, exhibits little or no hesitation in turning and moving directly toward the nest.

The possibility of vision of the entrance to the nest, of heliotropic orientation, or of wind influence, severally or in combination, can be perfectly excluded from the rôle of directive agents in this matter. Neither can the wetness nor dryness of the substratum or of the animals' tissues be important, for the behavior of the snails is the same during a dry and torrid afternoon as in a drenching down-pour of rain.

In an intensively studied situation where a number of Onchidium nests were found close together (30 to 50 cm. apart), it was seen that an Onchidium of one community so placed as to creep across the tiny sunken gully followed by the members of a neighboring colony would sometimes after hesitation, take this path and follow it for some centimeters; but in only one out of ten such instances did it actually *enter* the foreign nest; always there was hesitation and a retracing of the path, combined in several cases with an encircling journey about the foreign entrance. Yet, neither on the natural surfaces which they frequent, nor on various artificial surfaces tested does an Onchidium evince any tendency to follow its own slime-track or that of another individual; nor does it 'favor' a wet or a dry surface, a rough or a smooth one, in any detectable way (as, for example, an earthworm does).

It may be considered that some form of contact irritability, perhaps resident in the oral lapets, is partially responsible for this behavior. Experimental tests of this point, which are difficult to secure, are not yet complete. If the path ordinarily followed in the immediate vicinity of the nest be scraped bare, or the rock at the very entrance to the nest be chiseled away, the Onchidia endeavoring to return there collect at the edge of the cleaned area and then wander about in its neighborhood until they are covered by the tide (and washed off the rock).

There are additional features, of the first importance: An Onchidium found returning to its nest may be carried 50 cm. away and placed on rock above high water level, where these animals never go naturally, and in at least half the trials the snail succeeds in getting back to its nest. If a nest to which the inmates have returned be broken open and the animals placed on the rock at distances up to 50 cm. from the former entrance, they have no serious difficulty in making their way to the location of the old home.[3] Onchidia from neighboring colonies, or from laboratory stock, make no attempt to enter such a nest.

We are therefore forced to the provisional opinion that an Onchidium returns to its particular nest by virtue of some internal condition, simulating memory of the position of this nest in terms of its surroundings, but independently of the guidance which may be afforded by mechanically directive features of the environment.

To the extent that the homing movements of Onchidium may be proved to involve associative memory, this snail may be placed in a series comprising such types as Chiton, Fisurella, Onchidium, and Octopus, all four of which, in a sense, exhibit homing behavior, but of increasing degrees of precision and complexity in the order of the arrangement here given. The further study of Onchidium, both for itself and in relation to these other mollusks, should give rise to some valuable conclusions.

*Summary.*—Onchidium floridanum lives during high tide in 'nests,' cavities in the rock containing a number of individuals, from which the mollusks at low water emerge to feed upon exposed shore surfaces. The individuals emanating from any one nest return simultaneously to that nest before the tide rises again, and during this return give evidence of homing behavior, which seems to depend mainly upon internal conditions akin to the remembrance of specific localities.

[1] Contributions from the Bermuda Biological Station for Research, No. 95.

[2] A more complete account of these observations, with supplementary material, will be found in our paper, 'On the behavior of Onchidium,' shortly appearing elsewhere.

[3] When a nest has two openings, the removed inmates, or some of them, may turn directly to the *second* opening and enter there, even if they have in the first instance employed the other entrance.

# GROWTH AND DURATION OF LIFE OF CHITON TUBERCULATUS[1]

By W. J. Crozier

Bermuda Biological Station for Research, Dyer Island, Bermuda

Communicated by E. L. Mark, September 30, 1918

The common chiton of the Bermuda Islands, C. tuberculatus L., passes all of its post-larval and adult life within the limits of the tidal zone. A thoroughly representative sample of the total chiton population is therefore obtainable, though with difficulty in some places. In certain specific locations it has been possible to study practically all of the chitons there existing. Individuals are of various sizes, ranging during the spring months from 0.6 to 10 cm. in length. The frequency distribution of the lengths of the chitons at each locality is of an obviously multimodal character. It seemed likely, then, that one could work out some of the main features of the growth and duration of life of this mollusk. Facts of this sort, of which in the natural history of marine invertebrates there is a conspicuous deficiency, are not only important in a purely biological way, but have also a bearing upon the problem of estimating in outline the general fertility of the sea. This paper embodies a preliminary report upon one phase of an investigation having to do with the ethology of the chitons, especially from the standpoint of analyzing their behavior under natural conditions.[2] In a subsequent paper it will be shown that the growth phenomena in chiton probably afford a basis for the consideration of important questions which it has been impossible to approach successfully in such growth-studies as have previously been made on some other organisms in the warmer seas (e.g., Madreporarians). In this paper data are summarized establishing mean growth-curves for chitons inhabiting the region of Great Sound, Bermuda; although the chiton environments comprised within this region, a semi-enclosed sound, are ethologically far from being of a strictly homogeneous type, they nevertheless agree in certain significant physical respects, wherein they differ from other classes of situations (e.g., shores washed by the ocean surf) where chitons of this species also live.

Each of the eight plates which make up the 'shell' of chiton is compounded of a superficial 'tegmentum,' containing sensory receptors (photoreceptors[2]), and of a more deeply situated 'articulamentum.' The central area of the tegmentum of each plate is marked by a series of more or less distinct transverse grooves, or 'growth-lines.' The number of these growth-lines, best counted upon the six intermediate plates, is constant upon the different plates of one individual. On the assumption that these growth-lines, analogous to the concentric markings of the scales and otoliths of teleosts,[3] are formed at the rate of one per year, it is possible to estimate the growth-rate of chiton. The probable correctness of this assumption is assured, (1) by the inspection, throughout the year, of the growth of the young chitons appearing after the close of the breeding season; (2) by the direct observation that a growth-line is formed during the winter period, and (3) by the coincidence of the findings based upon the counts of growth-lines with those derived from the modes in the frequency-distribution of sizes in the chiton population.

The shell-plates are eroded at a characteristic rate, depending upon the particular environment, the erosion tending to obliterate the growth-lines. This difficulty in making the counts can, however, be overcome, and the measurement of the rate of the erosion itself affords an additional check upon the estimations.

From the region of Great Sound 1090 chitons were examined; of these the ages of 881 (including 476 mature males and 352 females) were determined from the shell-markings. The growth-curve (*A*) in figure 1 was constructed from the modes of the lengths in the respective age classes. These modes were closely coincident with the associated means. The data are heterogeneous to the extent that chitons from different situations within Great Sound have been lumped together. The mean growth-curve for either sex is sensibly the same. The form of the mean growth-curve—exhibiting an initial increase in growth-rate followed by the appearance of several overlapping 'cycles' of growth (Robertson)—is similar to that which may be calculated from the markings upon the shell of a single individual (fig. 1, *B*).

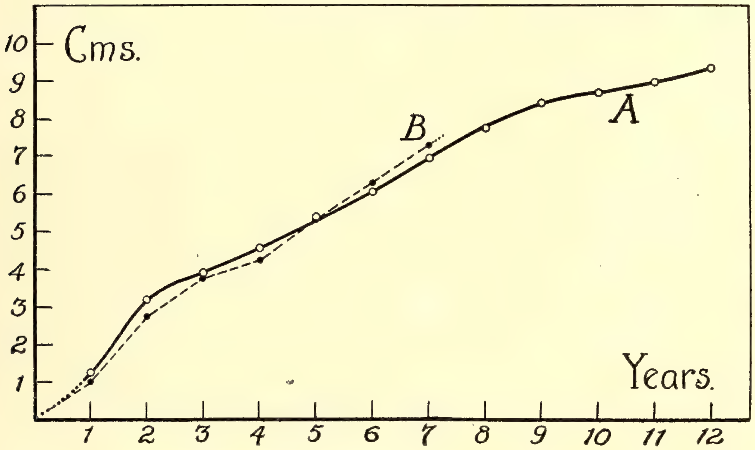

FIG. 1

FIG. 1. *A*, The modal lengths of Chiton tuberculatus at estimated ages one to twelve years; *B*, the growth of a chiton seven years old, as calculated from its shell markings. Great Sound, Bermuda.

The temporary depression in the rate of growth which commences with the third year is coincident with the onset of sexual maturity. This agrees with the similar correlation to be inferred from some brief statements by Heath[4] concerning the growth of several other genera of chitons.

It is of interest to determine the duration of life of chiton in different localities. In figure 2 are plotted the percentage frequency of the ages observed for chitons of both sexes. Chitons less than four years old are difficult to obtain in large quantities, because they live in crevices which are in many cases practically inaccessible. Therefore it is legitimate to consider only those individuals which are estimated to be more than five years old. Beyond this age, C. tuberculatus tends more and more strongly to come out into exposed situations. Therefore the relative proportions of the age-frequencies observed beyond the six to seven year-class are probably very nearly correct. Defining the 'mean death point' as the age at which there are already dead 50% of those chitons found to attain the age at which dying-off begins, it is seen (fig. 2) that the natural termination of life comes on the average during the eighth year. This agrees, in general, with the estimated average age of shells found naturally dead.

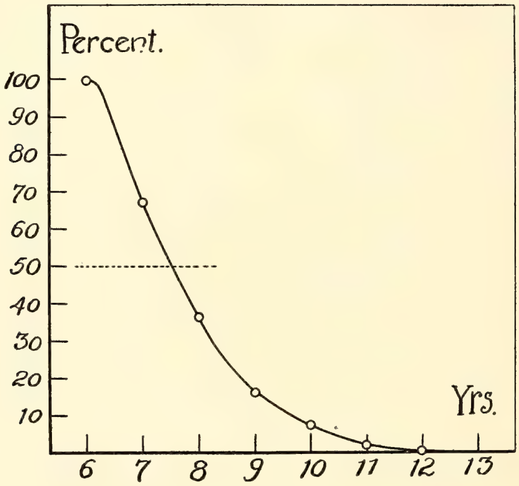

FIG. 2. THE PERCENTAGE SURVIVAL OF CHITONS AFTER THEIR SIXTH YEAR

Based upon records of 516 individuals six years old or over according to the indications of the shell markings. The 'mean death-point' is defined as that age at which 50% still survive; for this locality it falls in the eighth year.

The growth-curve established for the region of Great Sound, Bermuda, may be used as a norm with which to compare the growth of Chiton tuberculatus in other localities. These observations might with advantage be extended to more southerly portions of the West Indian faunal region, where Chiton tuberculatus is also found.

*Summary.*—A curve of the growth of Chiton tuberculatus has been obtained for the population in one locality at Bermuda, on the assumption that the age of a chiton may be estimated from the growth-lines upon its shell.

The mean duration of life is probably a little less than eight years. This curve affords a basis for comparing the growth of chitons in differing environments.

[1] Contributions from the Bermuda Biological Station for Research, No. 96.

[2] Cf. Crozier, W. J., *Amer. Nat., New York,* (in press), and Arey, L. B., and Crozier, W. J., (in press), for an account of observations forming the starting point of these investigations.

[3] Winge, Ö., *Medd. Komm. Havunders., Fisk., Kobenhavn,* **4,** No. 8, 1915.

[4] Heath, H., *Zool. Anz., Leipzig,* **29,** 1905, (390–393).

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## *GROWTH OF CHITON TUBERCULATUS IN DIFFERENT ENVIRONMENTS*[1]

By W. J. Crozier

Bermuda Biological Station for Research, Dyer Island, Bermuda

Communicated by E. L. Mark, September 30, 1918

In the preceding paper a mean growth-curve for the chiton population of Great Sound, Bermuda, was derived from the examination of 1090 individuals, on the assumption, supported by collateral evidence, that the age of a chiton may be estimated from the markings upon its shell-plates. It was pointed out that curves of this kind might be used to study variations in the growth of Chiton tuberculatus in differing environments. A test of this suggestion is afforded by the comparison of the rates of growth and of the average duration of life of this species when living (1) in Great Sound, (2) on the south shore of Bermuda.

Even within the limits of Great Sound itself the chitons inhabiting the shores of the several islands may exhibit differing rates of growth, as indicated in figure 1. When two such islands as Long Island and Grace's Island are compared, it is found that the chitons from the former place grow somewhat less rapidly than do those from the latter, although the initial growth-rates are identical. Most of the Long Island chitons occur on the north shore of that island, which is a cliffed shore with many caves, sheltered from the sun but exposed to the beating of the sea during northwest winter gales. Grace's Island is more sheltered from storms.

On the other hand, chitons from an exposed beach on the south shore of Bermuda, such as that at Cross Bay, which supports a more numerous chiton population than do situations within the stiller waters of the Sound, grow at first more rapidly, but after the first year are smaller, as compared with those living in Great Sound.

The physical differences between two such environments as those presented by Long Island and by the beach at Cross Bay, include at least the following: the sea-water on the south shore is more alkaline, contains more oxygen, and is lower in temperature; the algal food for the chitons is not only specifically less abundant on the south shore, but there are also relatively more of the chitons requiring it; at Long Island more shelter is afforded, in crevices, from the sun's heat at intertidal periods; at Cross Bay the older chitons, which live

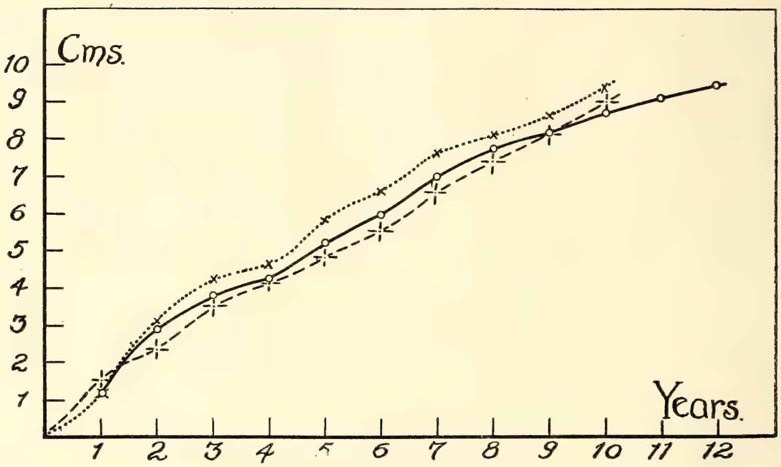

FIG. 1. THE RELATIVE GROWTH OF CHITON TUBERCULATUS IN DIFFERENT ENVIRONMENTS

——o—— Long Island, Great Sound (422 individuals). ······×······ Grace's Island, Great Sound (81 individuals). ——+—— Cross Bay, South Shore (591 individuals).

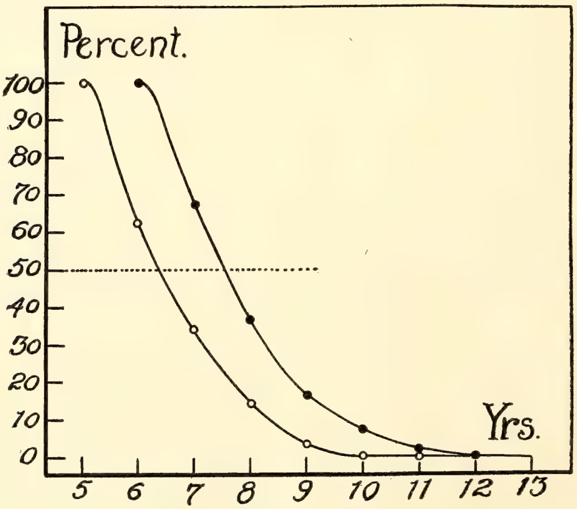

FIG. 2. THE PERCENTAGE SURVIVAL OF CHITONS ATTAINING THE AGE AT WHICH DYING-OFF BEGINS

● = Great Sound (515 individuals); ○ = Cross Bay, south shore (396 individuals).

further down the beach than do the younger ones,[2] are frequently covered to a depth of a foot or more by sand washed up by the tidal surf, and especially at times of southerly storms. The coöperation of these several differential influences is reflected in the fact that the chitons at Cross Bay die more quickly than do those in Great Sound (fig. 2).

The death rate is not the same for the two sexes. According to the results embodied in figure 3, the female chitons, on the whole, tend to die off more rapidly than do the males. This results in a preponderance of males among the oldest members of the population.

Independently of this effect, however, there are apparent in the present data some curious variations in the proportion of the sexes within the successive age-groups. The fact that these variations have been found in two distinct localities favors the view that one is not dealing with chance fluctuation. With one exception[3] the Placophora are, so far as is known, dioecious, and in Chiton tuberculatus the sexes are readily distinguishable. To what extent the fluctuations exhibited in the sex-ratio are due to environmental influences must be decided by further observation.

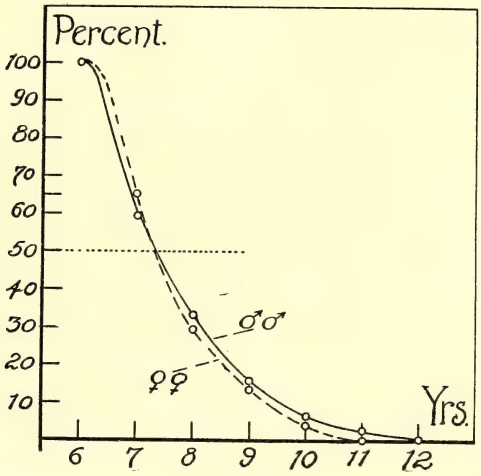

FIG. 3. THE RELATIVE DEATH-RATES OF MALES AND FEMALES (FROM RECORDS OF 912 INDIVIDUALS)

The sex of 1518 chitons was determined by dissection. The percentage of males ($R_♂$) in this series was 53.3; 1009 chitons from the region of Great Sound gave a ratio $R_♂$ = 54.9; 509 from Cross Bay, a ratio $R_♂$ = 50.4. With the data arranged to show the proportion of males at each age, in these localities, a progressive variation is evident (fig. 4). At first, there is a considerable preponderance of males, which, with fluctuations suggestively parallel in the two cases, is gradually decreased, until the differential death-rate becomes effective.

The chitons above seven years of age, which tend to occur in small groups scattered along the shore, exhibit a preponderance of males. Therefore it probably comes about, as a rule, that the eggs of a given female of this age or older stand a good chance of being fertilized, since solitary individuals of maximum age are more apt to be males. Whatever of adaptational effect there may be detected in this arrangement is probably, however, not of great importance. The major share of the reproductive activity of the chiton population is carried on by individuals between five and seven years old; they are more numerous than the older ones, and the volume of the gonad is in them relatively greater than at other ages. The proportion of the sexes is here very nearly as 1:1. These animals are usually found close together in groups of a dozen or more, under stones or in crevices.

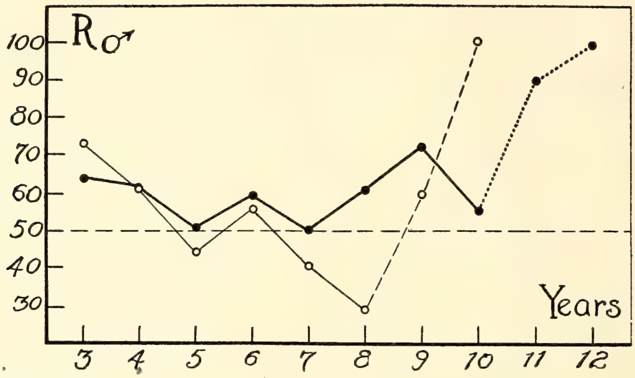

FIG. 4. VARIATIONS IN THE PROPORTION OF MALES IN DIFFERENT AGE-GROUPS OF CHITON TUBERCULATUS

● = Great Sound; ○ = Cross Bay.

*Summary.*—Curves have been derived which illustrate the extent to which the rate of growth and the duration of life of Chiton tuberculatus may be modified in differing natural environments at Bermuda; attention is also called to variations found in the proportion of the sexes, in different year-groups.

[1] Contributions from the Bermuda Biological Station for Research, No. 97.

[2] Cf. Crozier, W. J., and Arey, L. B., *Amer. Jour. Physiol., Baltimore,* **46,** 1918, (487–492). Arey and Crozier, (in press).

[3] Heath, H., *Zool. Anz., Leipzig,* **32,** 1907, (10–12).

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## *THE INTERFEROMETRY OF VIBRATING SYSTEMS*

BY C. BARUS

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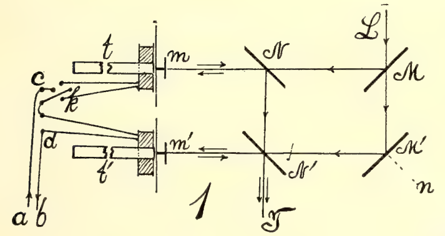
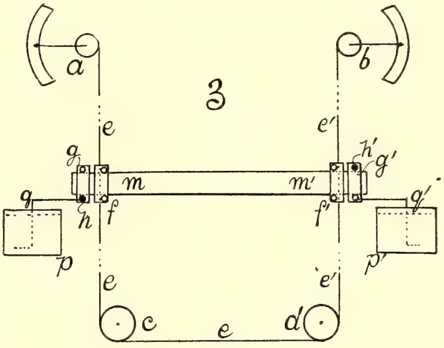
Communicated, September 6, 1918

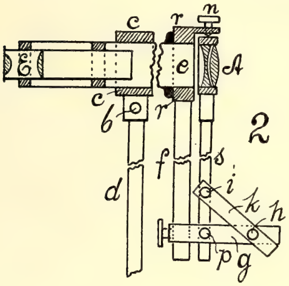
1. *Introductory.*—The high luminosity of the achromatic interferences and the occurrence of but two sharp fringes, make it possible to utilize them even in cases when the auxiliary mirrors vibrate. Experiments of a similar kind were

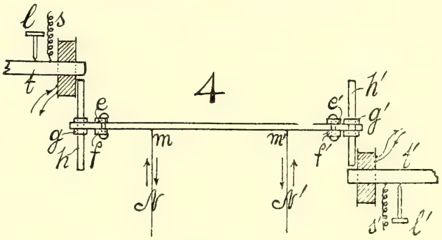
tried with telephones and the spectrum ellipses before;[1] but these fringes do not easily admit of being drawn out into a ribbon and there is usually deficient light.

2. *Apparatus.*—I began the work with two similar telephones, as shown at $t$, $t'$, in figure 1. Small mirrors were rigidly attached to the center of the diaphragms and each of the telephones secured on standards which admitted of adjustment around vertical and horizontal axes. The intermittent current was supplied at $a$, $b$ from a small induction coil with a rheostat in circuit. Four clamp screws at $c$, $d$, were available for putting the telephone bobbins in series or in parallel. The current of one could be versed at $k$. White light, $L$, from a collimator, was reflected or transmitted by the half silvered mirrors, $M$, $M'$,









$N$, $N'$, of the interferometer and from $m$, $m'$, on the telephones as indicated by the arrows. $M'$ was on a micrometer with the screw on the direction, $n$. To facilitate the finding of the fringes one of the telephones $t'$, should also be on a micrometer with the screw normal to $m'$. The fringes when found are observed by the vibration telescope at $T$.

The vibration telescope is shown in vertical section in figure 2, with the ocular at $E$ and the objective originally as $e$, the tube being supported on the standard, $d$, and clamp $cc$, admitting of raising and lowering and of slight rotation around the horizontal axis, $b$. The objective, $A$, has been removed and is now supported by a flat steel spring, $s$, in front of its former position.

In order that the objective may vibrate parallel to the fringes and as these appear in all angles of altitude, the special vibratory system $f$, $g$, $k$, $s$, has been devised. The rod *f*, reaching downward (about 10–15 cm. long) is attached to a ring of the objective, *rr*, capable of revolving with friction around the tube of the objective and of being fixed for any position in altitude of the rod. The horizontal clamp, *g*, adjustably attached to the rod, *f*, supports the spring, *s*. This passes through a vertical crevice in *g*; it is fixed by the set screw, *p*, and the two oblique adjusting strips, *k*, on either side of *g*. These pieces, *k*, are clamped by the screws at *h*, and serve as holders of the two coaxial set screws at *i*, on either side of *s*, so that the objective *A*, may be centered relative to the tube, *Ee*, for any oblique position of *f*, which must be normal to the fringes. The vibration may frequently be considerably changed by sliding *f* and *s* in *g* and reclamping the system. If the objective is to be fixed, a screw at *n* may be depressed for the purpose.

Under all circumstances the two spots of light representing the slit, if caught objectively on a screen at *T*, must be nearly coincident, horizontally and vertically, when the rays at *T* are parallel. If these spots are too far apart the fringes will be very small or even absent.

3. *Observations with telephones.*—The use of two telephones soon showed itself to be unsuitable for the present purposes, for the diaphragms oscillate not merely fore and aft (as in here desirable) but around horizontal and vertical axes as well, particularly when the vibration is relatively intense. Hence the coincident slit images of the silent telephone periodically separate. This shows itself in a peculiar manner in the field of the vibration telescope. The whitish field carrying the fringe waves due to the fore and aft motion of the mirror, *m*, *m'*, figure 1, on the diaphragm of the telephones, is intersected by nearly equidistant vivid white lines, normal to the waves. The latter are apt to be broken and appear only as traces between the vertical lines in question.

To account for these occurrences of the lines, it is sufficient to recall that the originally coincident identical fine slit images are vibrating through each other in some direction effectively normal to the slit. Hence these images will be seen clearly only at the elongations, when the slit images are temporarily stationary and at a maximum distance apart. Hence also the waves are absent at the lines, for here the slit images are so far apart as to eliminate the interferences.

As it made little difference how the telephones were electrically connected the diaphragms were removed and replaced by the two long strips of steel made from hack saw blades rigidly attached to the body of the telephone by a metallic arch. To approach this as near the magnet as possible forcing screws were provided at a little distance from the end of the strip. In this case the vibration of the strip and the mirror at its middle was fore and aft only and as a consequence the bright lines intersecting the field vanished completely. The arrangement of telephones, whether in series or parallel made a decided difference in the amplitude of the waves, which could be increased many times the breadth between successive fringes before the waves became turbulent and broke up.

*4. Bifilar systems.*—To utilize such a system as figure 1 to advantage it would be necessary to attune the elastic vibrators at $m$, $m'$, to the same period, which should be as nearly as possible identical with the period of the source of intermittent or alternating current. As an earth inductor or a small magneto inductor (single magnet rotating in a flat coil) would have to be used in the latter case, it seemed best to convert the apparatus into a bifilar vibrator as shown in figures 3 and 4 in elevation and plan. Here $mm'$ is a strip of thin mirror plate glass, about 32 cm. long, 1 cm. broad and 2 mm. thick, horizontal and in a position to receive the rays $N$, $N'$ of the interferometer (compare figure 1). Motion of $mm$ parallel to itself, fore and aft, will therefore produce no effect on the fringes; but any rotation around a vertical axis will be immediately apparent.

This strip of glass is supported by the bifilar system $e\ e\ e'\ e'$, made of a single thin wire. The ends of $ee'$ are wound around the horizontal screws $a$, $b$ which rotate with friction and are supplied with an index and scale, so that any amount of tension may be imparted to the wire. This passes below under the pulleys $c$, $d$, as nearly free from friction as possible, with the object of securing the same tensions throughout $e\ e'$. Flat clamps $f$, $f'$ of fibre and screws, attach the strip $mm'$ to the wire at any height, but necessarily near the middle of the vertical threads, where it receives the rays $N\ N'$.

The telephonic system consists of the horizontal soft iron screws $h$, $h'$ similarly attached to $mm'$ by the flat fibre clamps $g$, $g'$, and the telephones $t$, $t'$. These were made of large flat files. The screws $l\ l'$ are used to approach the telephone magnets $t$, $t'$, as near the soft iron armatures $h$, $h'$ as possible, without overstepping the unstable position, in view of the tension of the tense wire $e$, $e'$. To give the vibrating system adequate damping, thin wires $q\ q'$ less that a millimeter thick, bent and dipping into lubricating oil in a small vat $p$, $p'$ suffice. The fibres $e$, $e'$ were about 45 cm. long and their distance apart about 29 cm. Their period and that of the vibrating telescope were made about the same, on the average about 0.2 seconds.

As a generator an earth inductor with a coil of wire 60 cm. in diameter was at first used but later replaced by a small magnetic inductor. It was turned by a small motor. To measure the average intensity of current a Siemens precision dynamometer was installed, indicating currents as low as one-tenth of an average milliampere. Periods from about 0.1 second to 0.3 second were available.

The three vibrating systems (mirror, telescope, alternator) thus all admit of an adjustment of their periods and these should be nearly the same if the elliptic system of Lissajous curves are to be obtained, which is the preferable case. A change of the tension of the wires $e\ e'$ in figures 3 and 4, or any adjustments at the telephones calls for a fresh search for fringes; but this is not difficult.

*5. Further observations.*—Without synchronism in the two vibrating systems (current and telephone) the motion of fringes obtained is practically inappreciable when average currents within the order of milliamperes are treated. As soon, however, as approximate synchronism is established, the sensitiveness of the apparatus increases enormously. It is best for this purpose to vary the period of the motor of the alternate current generator by the slide rheostat If the fringes are horizontal and the objector therefore vibrating horizontally across the vertical slit image, the motion of he fringes is vertical. Hence the horizontal band of fringes in the absence of current, at once takes the form of a given Lissajous succession, with oppositions of rotations quite visible. The continuous change of these may be most conveniently accelerated or retarded by controlling the motor of the alternator with a slide rheostat. It is annoying if they leave the field while executing their gyrations, though they may always be restored by moving the micrometer. For mean tensions, the higher vibration curves 2 : 3, 3 : 4 may be obtained, both for the alternator moving at smaller and at larger periods than the vibrating mirror. To obtain them the motor running at maximum speed is gradually slowed down by means of the rheostat, when the forms appear in succession passing through the elliptic series at mean speeds.

The Lissajous curves continue to be very marked when additional resistances as high as 10,000 ohms are put into the circuit of the alternator. To obtain some idea of the smallest average current appreciable the Siemens dynamometer was put in circuit. Estimating the average current as $i = C\sqrt{\varphi}$ where $C$ is the dynamometer constant and $\varphi$ the deflection in centimeters, the current when 10,000 ohms is inserted and the magneto reduced in speed to $T = .25$ second was found to be of the average value of $i = 6 \times 10^{-6}$ amperes.

In a later and more refined adjustment the ellipses obtained with an insertion 10,000 ohms filled (as to their vertical or current axes) fully one-quarter of the field of the telescope. As little as one-tenth to one-hundredth of this would be easily appreciable with certainty, so that the minimum average current capable of detection may be estimated as well within $10^{-6}$ ampere. In this respect the device was somewhat disappointing. It must be remembered however that the above mirrors ($mm'$) and appurtenances are unnecessarily heavy, and the bifilar too robust.

If one of the telephones is reversed, the fringes which showed marked vibration in the first position, frequently *ceases to show any vibration* until the ellipses in the former case are very large (small resistance in circuit). The results in such a case are uniformly consistent. The reason of this is apparent: For when the telephones are joined in series the lever $mm'$, figure 3, is periodically rotated and released around a vertical axis and the displacement of fringes is proportional to the small angular amplitude of rotation. If, however, the telephones are connected differentially, the mirror $mm'$, if properly adjusted, merely moves parallel to itself, fore and aft and the fringes remain stationary. More usually, however, there is a difference in the size of ellipses in the two cases.

It is for the investigation of this question that the adjustment pushing screws $ll'$ and springs $ss'$ (fig. 4) pulling toward the rear, the telephones being on a vertical axis, were provided. If one of these, $t$ for instance, is placed in a definite effective position, while $t'$ is relatively far from its armature $h'$, the screw $l'$ may be gradually pushed forward diminishing the distance to the minimum. The effect of this is further to rotate $mm'$, and if the turning of $l'$ is cautiously done the fringes may be passed from top to bottom of the telescopic field by $l'$ and restored to position by the micrometer. In this way it is possible to find the tension of $ee'$ in which for one position of the commutator there is excessive motion of fringes whereas for the other there is practically no motion.

Adjusting the bifilar as to tension, there is one position or distance between $h'$ and $t'$ pretty sharply determinable, for which the fringe bands change to stationary ellipses in the *absence* of all current. This peculiar result is at first puzzling, but since it is quite synchronous with the period of the telescope (stationary ellipse), it is obvious that the motion of the objective is the cause of the phenomenon and that the fibers are now in unison with its period. For distances $h'$, $t'$, greater or smaller the ellipses soon return to bands. The effect of the alternating current on the stationary ellipse is very beautiful. It now oscillates very much like a smoke ring for one commutator position, whereas it passes in an accentuated way through all phases for the other.

Utilizing the preceding adjustment giving ellipses and bands respectively in the two positions of the commutator, many experiments were made to detect a change of phase when a large inductance is placed on both sides of one of the telephones. But in none of the experiments thus far, was any difference discernable to be attributed to the presence of the inductance.

As a tuned system responding to definite periods only, the vibration interferometer is quite sensitive, provided the average currents are of the order of several microamperes. Between the types of compound vibration curves corresponding to frequency ratios of 4/3, 3/2, 1/1, 2/3, 3/4 there is usually an unbroken band of fringes. If the ratio of periods remains fixed, the vibration curve of course remains fixed, which is the usual sharp acoustic criterion. When the ellipses (out of tune) change continuously between lines of different inclination, the passage in one direction is often gradual whereas in the reverse or return direction it is almost sudden. Linear forms flop into linear forms as it were. No doubt this is related to the vibration of a bifilar system like the above, where the two ends are liable to vibrate alternately. When the average currents approach the order of $10^{-4}$ am., the bands became waves for all periods not excessively high or low.

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[1] *Washington, Carnegie Inst., Pub.*, No. 149, pt. 3, § 123–125, 1914; cf., *Amer. J. Sci., New Haven*, **3**, 1897, (107).

# ON THE ESSENCE OF PHYSICAL RELATIVITY

By Sir Joseph Larmor

Cambridge, England

Communicated, September 19, 1918

In an important investigation recently reported,[1] Mr. Leigh Page has determined an expression for the translatory force required to sustain an assigned varying velocity in an electrostatic system of a certain type, that namely which it has been usual to investigate as a model of an electron in analytical discussions. The expression contains terms involving the acceleration of the system and its time-gradients, but no term involving its velocity except in combination with these other quantities. The application of his formula to which Mr. Page restricts himself is to challenge a result that an isolated radiating system is subject to retarding force from the reaction of its own radiation of amount equal to its velocity multiplied by rate of radiation divided by $c^2$. The necessity for such refutation is based on the idea that on principles of relativity the velocity of an isolated body could have no meaning. But the formula obtained seems to leave this question as it was: for equally the acceleration of the isolated body could have no meaning; and moreover, though the thrust of the radiation is, in this case compensated, the velocity appears to be actually involved in the formula in the same manner as the impugned result would require for this particular problem of a convected electrostatic system, for which the radiation is extremely transient and very slight in the absence of extraneous force.

It has always seemed to me that this subject, which may be described as that of interaction of the æther with uniform motion, though of slight account phenomenally, is theoretically of high significance, in that it is destined perhaps to throw light on the nature of the forward momentum that is convected by radiation, and thence on the intimate dynamical nature of radiation itself and the physical function of the æther. I therefore propose briefly to indicate what I hold, provisionally of course, to be their present trend of knowledge. An adequate survey of this widely ramifying domain, in its various aspects, is difficult to compress, even when concentration and avoidance of detail is the main aim, and is therefore reserved for a communication to the *Physical Review*.

In the first place the plan now customary of regarding an electron as merely an aggregation of differential elements of electricity seems to be far too narrow a foundation for its internal properties, except by way of partial and provisional illustration. If electricity is to be explained as constituted of electrons, it can hardly be a step towards finality to explain electrons as constituted of electricity. On such a view the kinetic reaction of the inertia of an electron presents itself in theory under the artificial guise of a self-retarding force exerted by the electron on itself, as given by Mr. Page's formula: moreover on this order of ideas there is no reason assignable why electricity should be aggregated into electrons at all, unless other forces and constraints extraneous to the electric theory are to be introduced to hold them together.

On the other hand an æther theory demands immediately the existence of electrons as the singularities in its own constitution—like vortex-rings in fluid—which determine its activity: and the electrons had already formed an essential part of electrodynamic theory before they were detected in the free state, and the actual magnitude of their inertia thereby ascertained in comparison with the masses of the atoms of the dynamical elements. As being thus self-subsisting mobile structures, whose interactions with each other at distances great compared with the dimensions of their nuclei are fully known, but whose internal constitution and adjacent influence could only be illustrated and guessed at, their dynamical specification will be most suitably, at any rate most hopefully, expressed in terms of their electric charge, their effective inertia, and their electrodynamic fields of known forms except inside and close up to the nuclei; all else being held in suspense as matter only for provisional speculation. As in the case of the simplest exemplar of a theory of a medium, the hydrodynamic theory for an ideal perfect fluid containing vortex rings or moving solids, this inertia may be aeolotropic: it may even when waves can travel be a function of the velocity instead of being constant. But it is to be the same whether the system which is its seat be moving with acceleration or not. The momentum of the system, of type $mv$, is not to involve its acceleration: nor is the energy of translatory motion of the body, of type $\frac{1}{2}m^1v^2$ where $m^1$ may be different from $m$. Otherwise all existing general dynamical theory would be dissolved. The production of acceleration is ascribed to applied force, which measured by its result as the value of $d(mv)/dt$. The formula for the force required to sustain an assigned varying motion of Mr. Page's special model of an electron does not in this way introduce only $v$ and its acceleration $f$: the terms in it which involve the gradients of $f$ have no place as yet in this scheme. Some origin extraneous to the electron must be assigned to them. On an æther-theory that cause is the radiation shot out from the electron while it is in varying motion, which convects somehow mechanical momentum away from it into the free state; the backward kick of this momentum acting on the source from which it is ejected is describable as the back-pressure of the radiation, which would of course exert a compensating forward kick on any obstacle, however distant, which absorbs it. However we may clothe our thought in language of relativity, it would appear that this issuing radiation does effectively possess an absolute velocity $c$, and therefore an absolute velocity of its source also is theoretically determinable from observations made in relation to it: and this seems to explain how it is that Mr. Page's formula for the force on a system entirely isolated can involve the acceleration of this system and its velocity also.

But a complete isolated system travelling with velocity $v$, when we include in the system all the radiation that has issued to however great a distance from its nucleus, cannot create new translational momentum. Therefore the backward pressure proportional to $v$ of the radiational momentum projected from it namely $-v/c^2.dE/dt$ on this theory, must be compensated by disappearance of forward momentum attached to the source itself. The momentum of the source is $mv$: its rate of change is $mdv/dt+vdm/dt$; the first term is the usual kinetic reaction of the inertia $m$ it has at the instant; the other term proportional to $v$ is the compensating one here adumbrated. It follows that we must admit change of mass specified by the formulae $\delta m=\delta E/c^2$; so that the mass of a radiating body diminishes proportionally to the energy it radiates, except in so far as there is compensation provided by extraneous radiation which it absorbs.

For example, a particle of cosmic dust describing an orbit round the Sun is retarded by its own radiation, but its mass is kept up by the radiation it gains from the Sun: thus there is no compensation to the resisting force in this case, and the particle will gradually be sucked into the Sun, as Poynting in an important memoir showed.

The physical theory of electrodynamics, including optics, would thus, on the so-called classical lines—which can be held to be not yet obsolete, though special problems of expansion or development to adapt them to new experimental discovery are pressing —involve electrons specified in terms of charge and field and inertia, which are the link with matter, and it would involve also free radiation existing in the æther, specified as regards mechanical force by its convected secondary longitudinal momentum, but physically in evidence mainly by the type and energy of its transverse undulations. These are the materials out of which the science is to evolve itself: and the problem is whether that evolution can be gradually effected in a natural and consistent manner, and one which runs parallel with the course of evolution of experimental knowledge.

The true essence of the relativity of external knowledge is that we can investigate a system only in relation to some other system, and the most convenient perhaps the only feasible other system has been hitherto the ideal Newtonian frame of reference of space and time; for that is the canonical system, so to speak, with regard to which dynamical principles take on an ideal simple form, and it is a system which is being determined with continually expanding precision by the progress of astronomical science.

The special question now in evidence is—Is it now expedient to exchange this frame of reference, corresponding to $c$ infinite, for another far more complex but very slightly different continuum having a finite space-time modulus $c$ ? The more fundamental question is—Are we to assign to either frame dynamical properties, typified by propagation of physical effect in space in terms of undulations sustained by stress and inertia, or are we to assign to it properties solely geometrical and regard all physical effect as merely projected in duration across space? The forms of special unrestricted relativity which have been recently current ultimately demand and perhaps prefer the latter course; and such modes of expression can apparently be elaborated so as to include most of optics, though perhaps in an artificial and unfruitful manner, if we replace the Newtonian scalar corpuscles of light by projected vector elements of fields of potential (not however conserved in value) from which forces would be analytically derivable. Mr. Page's work has improved very remarkably such a scheme of projected influence by showing that, provided *c*-relativity is postulated for space and time, it is elements of longitudinal electric force that may be regarded as projected from the sources, and are moreover conserved after the manner of *quanta* as they travel onward.

There is no absolute criterion to decide between the two ideals. The first order of ideas has proved itself as the foundation on which the interlaced fabric of electric and optical science has been actually constructed: the other seems to offer as yet only somewhat ingenuous and disjointed though significant expression for certain striking features of recent discovery which the former has not yet succeeded in assimilating, and seems to require us to obliterate the course of evolution of the science or perhaps to retain it as a mere historical survival.

All these modes of restatement of departments of physical science in more expressly relative terms may be comprehended as partial analytic developments of the far wider principle of the purely relational character of our external knowledge, which was advanced and systematically fortified with great abstract force in the general metaphysical domain by Bishop Berkeley; a principle by means of which he passed on to examine the criterion of real objective existence, and one which was well understood in its present aspect by his friends in Yale College nearly two centuries ago.

Thus in these matters we are hardly concerned with refuting any theory, for all are relative: it is fruitless to traverse any proposition, unless we take into account the definitions and context in which it subsists. The question is, as to which scheme of formulation gives as a whole the closest and most expressive representation of the complex of natural knowledge, and affords the most promising clue to its future elaboration and extension. But a choice does not by any means preclude development along other promising but provisional lines for which an interpretation has yet to be found. A main merit of Mr. Page's powerful papers, especially that of 1914, the feature which may be said to constitute them into a theory, is to my mind that they translate the usual so-called classical electrodynamic theory into the order of ideas, formulation under relativity of the *c* type being an essential feature, that describes physical action in terms of projected entities (which might perhaps even be *quanta*) of various kinds, effecting this translation in more intuitive and fundamental terms than had previously been attained to; and also that by their conciseness and geometrical directness they facilitate that comparison and contrast with the alternative order of ideas which is the essential matter.

[1] These PROCEEDINGS, **4,** 1918, (47–46); *Physic. Rev., Ithaca,* (Ser. 2), **11,** 1918, (376–400).

# GRAVITATIONAL ATTRACTION IN CONNECTION WITH THE RECTANGULAR INTERFEROMETER

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*1. Introduction.*—The ease with which the rectangular interferometer admits of the measurement of small angles, induced me to adapt an apparatus with reference to it, for the measurement of the Newtonian constant. In addition to the usual system suspended in air, I also tested a floating system. Accurate work is scarcely to be expected in this laboratory where temperature variations and the agitation of the room conflict with the condition of its obtainment. But the trial is nevertheless interesting and the final work may be done elsewhere.

*2. Apparatus.*—There is room here only for the well known quartz fiber method of Boys, using a light needle, weighing when loaded but 1.9 grams. This was made of a rigid shaft of straw *mm*, figure 1, 26 cm. long, the ends being split slightly into four symmetrical segments each, which receive the two shots *m* and *m′* additionally secured with a little wax. The two light mirrors *b* and *c* were differently mounted: *b* on a fine pin *d*, snugly fitting corresponding perforations in the straw, was thus capable of rotation around the vertical axis (*d*) and moving up and down slightly. The mirror *c*, however, was mounted on a thin elastic strip of aluminum, clasping the shaft as shown in section at *e′*, *c′*. This is thus capable not only of rotating on a horizontal axis, but of being placed at different distances (moving right and left) from *d* to accommodate the rays of the interferometer, to which *b* and *c* are to be normal.

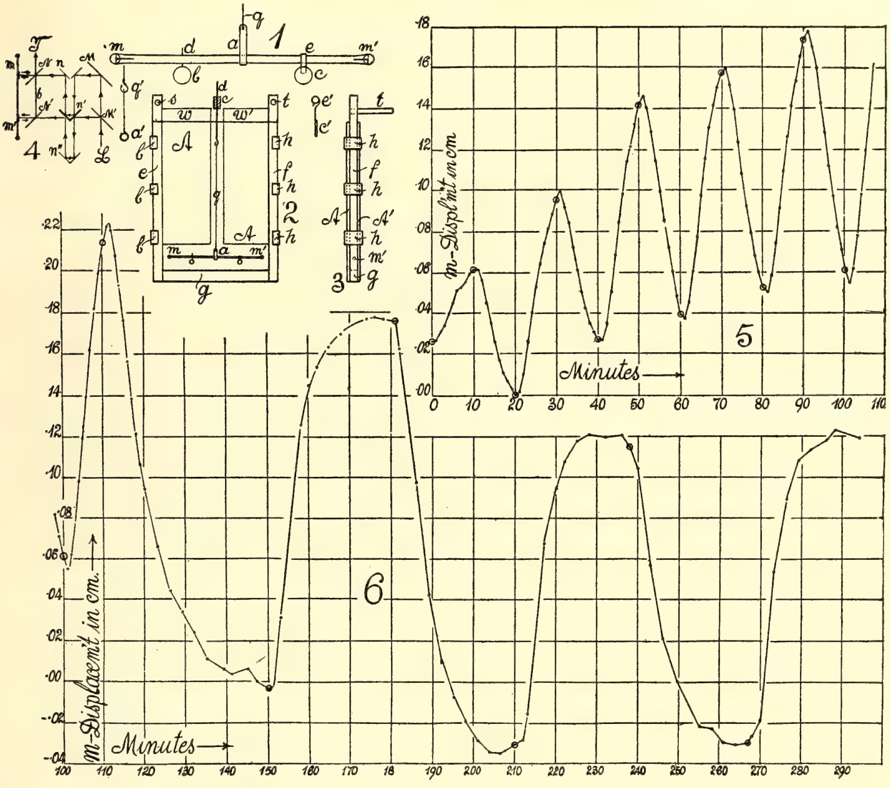
The needle is swung from a quartz fiber *q*, and a strip or hanger of elastic aluminum, *a* (see *a′*, *q′*), which clasps the shaft. Hence the latter may also be moved endwise to be balanced and rotated around a horizontal axis, till *b* and *c* meet the rays normally.

These operations are completed by trial before the needle is definitely hung, preferably in a broad beam of sunlight. No great accuracy is required.

The shallow case is made of two plates *AA*, figures 2, 3 (side view), of plate glass about 32 cm. broad and 45 cm. high, spaced by two strips *e*, *f*, 3 cm. wide of wood 1.2 cm. thick, reaching from top to bottom. Six steel clips, *b*, *b*, *h*, *h*, (such as are used for binding pamphlets), held these strips in place. At the projecting tops of the wooden strips, two nipples *s* and *t* of $\frac{1}{4}$ inch gas pipe fixed normally to the strips, served for the hanging of the case and needle from a firm wall bracket. The bottom of the case is subsequently to be closed from below by a strip of felting *gg*. To diminish the space within the case, two thin cloth covered wooden boards *w*, *w*, are inserted from the top.

The quartz fiber $q$ to carry the needle $mm$, hangs from a long one-eighth brass rod $d$, which may be raised or lowered in view of the sleeve $c$ held by a separate adjustable arm, without. The rod $d$ must fit the perforation in the cork nicely, so that the former may be smoothly raised or lowered and held in any position in virtue of friction.

To swing the needle this is first placed on cork Y's, below the opening of the case $AA$, the felting $gg$ having been removed. The quartz fiber $q$ is then lowered on the long stem $d$, until the lower hook on the fiber is in position to grasp the clasp on the needle $mm$ still below the case. The needle is then cautiously lifted by raising $d$. After this the felt strip $gg$ is inserted to close the case, and the necessary adjustment made at $c$ and $d$ to swing the needle freely in the restricted space provided for it.



In the rectangular interferometer used (fig. 4) the breadth of the ray parallelogram was $b = 10$ cm. and the angle of incidence $i = 45$ degrees, so that if the micrometer displacement $\Delta N$ corresponds to the small twist $\theta$ of the needle, $\theta = 0.071\ \Delta N$.

3. *Observations.*—The quartz fiber used (diameter $2\,r$) was $L = 17$ cm. long. Supposing the distance apart of attracting weights $M$ and attracted weights

*m*, to be $d = 6$ cm., and $M = 10^3$ grams, if the rigidity of quartz is taken as $5 \times 10^{11}$, $\gamma = 6 \times 10^6 r^4 \Delta N$. Thus if

$$r = 10^{-2} \text{ cm.}, \quad \Delta N = 1.1 \times 10^{-6} \text{ cm. per kg. of } M, \quad \text{Period } T = 4.6 \text{ sec.}$$
$$r = 10^{-3} \text{ cm.}, \quad \Delta N = 1.1 \times 10^{-2} \text{ cm. per kg. of } M, \quad T = 465 \text{ sec.}$$

The first case would then correspond to but 1/40 of a fringe; in the second case there should be 250 fringes per attracting kilogram. The working conditions below proved to be much better than this, rigidity being estimated too high. If the tenacity of quartz be taken as $1.5 \times 10^3$ kg. per square centimeter the latter filament ($r = 10^{-3}$ cm.) should still hold 4.5 grams, or much more than the weight of the above needle (within 2 grams). Having mounted the needle and found the fringes, the image of the wide slit (i.e., the reflected beam seen in the telescope) was apparently quite stationary, the light needle being thus adequately damped. But within this virtually stationary slit image, the fringes (preferably made horizontal) during the winter months continually wandered up and down, showing that micrometric vibration had not been eliminated. The experiment is a very impressive one; but the drift in a heated room is so large that all attempt at measurement is idle.

In the summer installation in a subcellar at nearly constant temperature with a few improvements of apparatus (the mirrors being readjusted, etc.) the needle was without difficulty made to take a stable position midway between the glass plates, subject to the torsion of the fiber. With the micrometer at 45 degrees, the latter is limited in its displacement. I therefore later attached a special micrometer with three identical pairs of parallel V-mirrors (the latter at 90 degrees) as shown at $n$, $n'$, $n''$, in figure 4, $M$, $M'$, $N$, $N'$, being the interferometer mirrors, $L$ the collimator and $T$ the telescope. This design when the mirrors are reciprocally parallel, has the additional advantage of being independent of slight changes of inclination in the micrometer. The displacement due to V-mirrors is now virtually parallel to the rays and no difficulty in finding the fringes need occur. Naturally the mirrors must be good, there being now four additional reflections in each ray; and the V-apparatus must be accurately adjusted for parallelism of mirrors. The central mirrors, $n'$, only are moved by the micrometer screw.

With the fringes found there is now no difficulty in showing the attraction of gravitation. In fact in iron brick moved on a small truck near the shot at one end of the needle, gripped the balls $m$, $m'$ very much like a magnet acting on the pole of a magnetic needle. Throughout the whole of the experiment the fringes were under the perfect control of the micrometer.

A more systematic experiment was made by testing the attraction of a lead ball 5.43 cm. in diameter and weighing about $M = 950$ grams, for the shot $m'$ (at the end of the needle) weighing $m' = 0.61$ grams. The kilo was moved on a circular track with stops to a distance of $R = 4.24$ cm. (between centers of balls) from the ball of the needle, alternately. The period of the air-damped needle can not have been less than 18 min. The case is then that of a forced vibration under constant force and a large logarithmic decrement. An example of the observations is given in the graphs figures 5 and 6, the reading being made every minute beginning with the equilibrium position ($M$ in the neutral position). The ordinates of the graphs show the displacement of the attracted shot, $m'$, in cm. The periods of alteration were 10 minutes during the first 110 minutes; thereafter the period of the needle was exceeded. Circles show the time of turning $M$, from one side of $m$ to the other. The approach to a limiting arc is regular during the first 100 minutes, after which some extraneous disturbance enters; but both for the longer and the shorter intervals of alternation, the tendency is toward the same limiting arc. At the end of the damped period there is a sort of fluttering. The excursions of the needle are so large that it was not thought necessary to read the fringes throughout. In fact, if a micrometer scale is put across the wide slit, there are two methods simultaneously available for finding $\Delta x$, the displacement of $m$ due to $M$. For if $f$ is the focal distance of the collimator and $\Delta y$ the reading in the telescope of the collimator micrometer scale specified,

$$\Delta x = \cos i\,.\,l\Delta N/b = \Delta y\,.\,l/2f$$

But the reading in terms of $\Delta N$ is enormously more accurate than the reading of $\Delta y$, the shift of the slit image in the telescope. If the motion of the needle were less nearly dead-beat, the attracting force could be computed from the limiting distance between elongations, if the torsion coefficient of the quartz fiber and the logarithmic decrement were known. From static experiments made during hour intervals this double amplitude was found to be 0.116 cm. or a departure of the shot $m$ at the end of the needle from its position of equilibrium of 0.058 cm. in response to the attraction of $M$. If $l$ is the semilength of the needle (between centers of shots), the micrometer displacement $\Delta$N and the displacements $\Delta x$ of the mass $m$ are given by the equation

$$\Delta x = l\Delta N \cos i/b = 0.89\ \Delta N$$

where $b$ is the breadth of the ray parallelogram and $i = 45$ degrees the angle of incidence of the interferometer. Thus the micrometer displacement is of the same order as the displacement of $m$, and if the latter is 0.116 cm., we should have

$$\Delta N = 1.3 \text{ cm.}$$

As the micrometer reads to $10^{-4}$ cm. 1/1300 part of the attraction between $M$ and $m = 0.61$ gram could therefore be detected; i.e., the attraction of 0.73 gram or per interference fringe well within one-third of this, for the given quartz fiber (which was not specially selected) and given distance $R = 4$ cm. This is equivalent to the attraction of 0.2 gram at a distance of 1 cm., per fringe.

Apart from the measurement of the torsion coefficient of the fiber, there is however a real difficulty involved, and that is the occurrence of marked drift in the needle. It is only incidentally that the fringes are found at rest. The chief contributory cause of this is no doubt the occurrence of motion of air around the needle provoked by small differences of temperature, resulting for instance from illumination. If the possible accuracy of deflection measurement is to be of any value, therefore, the apparatus must be kept in the dark except during observation. Fortunately the achromatic fringes require little light. Even then a closed case which can be exhausted of air is essential, for such radiometer forces as may enter would in any event be differential, seeing that the mirrors are symmetric and the illumination is not subject to alternations like the forces on $m$. It is probable that in case of the above regular method a thicker quartz fiber and a greater distance $R$ would conduce to the best results since $\Delta N$ is the least difficult quantity to determine.

The question may now be asked whether it is not possible in the present case to determine gravitational attraction in terms of the acceleration of balls resulting. With an ocular micrometer this would not be difficult as the fringes move slowly enough that the position can always be sharply specified.

But with a needle of long period in vacuum, the screw micrometer would also be available. If there is no damping we may write

$$\gamma Mm/R^2 - ax = 2m\alpha$$

where $\alpha$ is the acceleration, and $a$ the torsion coefficient, referred to $x$ the displacement of $m$, at the time $t$, supposing the needle starts from rest, and the gravitational force is applied at $t = 0$. If at the outset we may put $X = \alpha t^2/2$

$$\gamma Mm/R^2 = X(at^2 + 4m)/t^2$$

an equation whose interesting feature is this, that if $t$ is kept very small (which should be possible with an ocular micrometer and the achromatic fringes, a fine quartz fiber presupposed) the term involving $t$ may be neglected and the experiment interpreted as a case of uniformly varied motion in which

$$\gamma = 2R^2\alpha/M$$

Thus for instance if $R = 5$ cm., $M = 10^3$ grams and $\gamma = 6.7 \times 10^{-8}$ and if $t = 100$ seconds is admissible, $\alpha = 1.3 \times 10^{-7}$ cm. /sec.$^2$ and the distance traversed in 100 seconds would be 0.0065 cm., well measurable on the interferometer, quite so if the work is done reciprocally and the interference fringes are used individually. The theoretical error will be a minimum if $m$ is as large as the fiber can safely carry and $t$ as small as possible. On the other hand $X$ is independent of $m$ and if $t$ is to be kept small, the result may be compensated in a large $M/R^2$. The measurement is thus to consist in keeping the fringes at zero by moving the micrometer screw for the small interval $t$ during which the weight $M$ acts. The constant $\gamma$ would then follow from the micrometer reading, $M$ and $R$ only, all other quantities entering secondarily, as corrections. The experiment seems well worth while.

# *THE GENERAL CHARACTER OF SPECIFIC HEATS AT HIGH TEMPERATURES*

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The wide variations presented by atomic heats at low temperatures have been brought under a single generalization, usually expressed in terms of the quantum hypothesis, in the light of which all follow a common law. This law still demands that at sufficiently high temperatures all atomic heats at constant volume should have the same constant value, approximately 5.96. The applicability of this theory, however, has been doubted; the supposition has been made that the atomic heats continue to increase with temperature, and this has been proved to be true of some metals. The present paper gives data for some non-metallic substances which show the probable existence of such an effect in every case. The substances in question are silica and various silicates. Their specific heats were determined by dropping relatively large charges (50 to 80 grams) from electric furnaces into calorimeters. Temperatures up to 1400° were used in three cases, and up to 1100° at least in most of the others.

The accuracy of the results determines the value of the evidence, and therefore needs attention.[1] (1) As to the 'accidental,' 'visible,' or continually varying errors, the discrepancy between duplicate measurements averaged under 0.5 per mille. Moreover, this small amount of discrepancy was largely accounted for by the irregularity of heat loss in dropping the hissing hot body into water, while this loss, except for its irregularities, was completely canceled out by the procedure used. (2) The unvarying, or systematic, errors were checked by using two each of practically every piece of apparatus, such as furnaces and calorimeters. Comparisons nearly always showed discrepancies under 1 per mille.

The temperature scale was established by means of one of the original Day and Sosman thermoelements, confirmed by numerous concordant metal melting points.

(3) The slowly or occasionally varying errors, the most insidious and detrimental, were checked by repeating some determinations at intervals of several months, with uniformly satisfactory results, and also by general comparisons as in table 1. Here about half the results are compared by giving their differences from a standard fictitious silicate which is approximately the mean of the whole. Of forty-five results above 0° to 100° only three, sporadic ones, those italicized, show discrepancies from a smooth curve exceeding, and then only slightly, the allowed accidental discrepancy of 1 per mille, and, what is perhaps more important, the different comparable series of values, pseudo-wollas-

tonite-diopside-enstatite, and anorthite-andesine-albite, show almost perfect concordance. The temperature distribution in the furnace, the most dangerous source of error, was studied with especial care, and the maximum irregularity detected corresponded to 0.3 per mille. The precautions against systematic error thus appear to be unusually numerous, and the conclusion seems warranted that most of the original results, for mean atomic heats over intervals, are correct to better than 3 per mille, of which less than 1 per mille is accidental.

The method of obtaining from these interval atomic heats the true atomic heats at single temperatures was demonstrably such that original *accidental* errors of 1 per mille might possibly produce single errors of 1% and would be certain to produce single errors of about 5 per mille in a few cases, but would not make any *series* of results wrong by much more than the original error.

TABLE 1

INTERVAL ATOMIC HEATS OF SILICATES, COMPARED BY BEING GIVEN AS DIFFERENCES FROM A FICTITIOUS STANDARD SILICATE

| | SILICATE, MINUS STANDARD | | | | | |
|---|---|---|---|---|---|---|
| | 0°–100° | 0°–300° | 0°–500° | 0°–700° | 0°–900° | 0°–1100° |
| $MgSiO_3$ Amphibole | 0.023 | 0.036 | 0.039 | 0.039 | 0.038 | 0.043 |
| $MgSiO_3$ Pyroxene | 0.036 | 0.059 | 0.084 | | | |
| Diopside | 0.108 | 0.109 | 0.108 | 0.109 | 0.109 | 0.107 |
| $\frac{\text{PW} + \text{Py}}{2}$ | 0.129 | 0.115 | 0.111 | | | |
| P. Woll. | 0.223 | 0.170 | 0.137 | 0.113 | 0.093 | 0.082 |
| Woll. | | | | 0.149 | 0.139 | 0.136 |
| Anorthite | 0.012 | (008) | *0.013* | 0.001 | 0.006 | 0.019 |
| Andesine | −0.055 | | −0.056 | −0.057 | −0.053 | |
| $\frac{\text{Anor} + \text{Alb}}{2}$ | −0.048 | | −0.048 | −0.056 | −0.052 | |
| Albite | −0.107 | −0.109 | −0.108 | −0.113 | −0.109 | −0.107 |
| Microcline | −0.096 | −0.114 | −0.112 | −0.112 | −0.116 | *−0.126* |
| Micro. glass | 0.006 | *0.003* | 0.013 | 0.017 | 0.021 | 0.063 |
| Standard alone | 4.067 | 4.558 | 4.913 | 5.143 | 5.316 | 5.453 |

*Results.*—Results for seven materials are given in table 2 as true mean atomic heats. Of the substances included, silica glass is known to have an almost zero expansion coefficient, hence its atomic heat, though obtained at constant pressure, is the atomic heat at constant volume, or $A_v$. Cristobalite, a crystalline form of silica, shows a still lower atomic heat, and also shows it up to a considerably higher temperature. Quartz, which also is lower, has a negative coefficient of expansion from 600° to 1000° and the very low value of $A_p$ for cristobalite may be partly or wholly a result of negative expansion. If the small thermal effect of the contraction of quartz is computed and taken into account the result brings $A_v$ up to its value for silica glass, within the uncertainty of the various data.

In order to interpret these results it is necessary to make allowance for the fact that according to the Kinetic Theory, as usually modified to fit the behavior of specific heats at low temperatures, $A_v$ for these oxides is appreciably below 5.96 even at 1300°. Any high-temperature variation must appear as a variation from the value calculated from the low-temperature results. Nernst,[2] using the Nernst-Lindemann formula, has derived a special formula for the atomic heat of silica glass based on determinations from $-247°$ to $+283°$, and this formula, slightly modified to agree with the present results at 283°, at which temperature Nernst had used an approximated value of Magnus', has been used to get the values given in line 5 of table 2.

These values, for 1100° and 1300°, are below the values for cristobalite by more than the probable error, which indicates that unless cristobalite changes very considerably its rate and probable direction of volume change near 1000°, $A_v$ for it is increasing with temperature above the accepted theoretical value. $A_v$ for silica glass is already well above the theoretical curve at 900°, and is probably still further above it at higher temperatures. Unfortunately, this glass could not be continuously heated above 900° without danger of crystallizing. Unlike nearly all the other series the original values for cristobalite were obviously not on a smooth curve, and were smoothed before being used to get the true heats. But none of the changes was above 1 per mille, the permitted accidental error. A very different plausible alteration gave identical results, and none which seemed reasonable gave a value at 1300° lower than 5.83 obtained from the unsmoothed data, and even this value is above the computed curve. For this curve the Debye formula, and even the original Einstein formula, give above 700° results practically identical with those in the table, so the conclusions are not dependent on the particular formula used. Since the theory applies to solid bodies only, and silica glass, being amorphous, has some of the properties of a liquid, the results for cristobalite are more sig-

TABLE 2

ATOMIC HEATS. $A_p$

| | 300° | 500° | 700° | 900° | 1000° | 1100° | 1300° |
|---|---|---|---|---|---|---|---|
| Silica glass | 4.95 | 5.35 | 5.58 | 5.75 | | | |
| Quartz | | | 5.46 | 5.66 | | | |
| Cristobalite | | | 5.55 | 5.67 | 5.72 | 5.78 | 5.86 |
| Silica glass, formula, $A_v$ | 4.95 | 5.35 | 5.55 | 5.67 | 5.71 | 5.75 | 5.80 |
| Albite | 5.10 | 5.46 | 5.71 | 5.91 | 5.97 | | |
| Microline | 5.09 | 5.47 | 5.72 | 5.86 | 5.92 | | |
| Anorthite | 5.22 | 5.58 | 5.82 | 6.04 | 6.14 | 6.31 | 6.82 |
| Platinum, $A_v$ | | 6.45 | | 6.79 | | | 6.84 |

nificant. The values for platinum, given for comparison, were obtained along with the silicates.

Albite and microcline have a smaller proportion of oxygen atoms than silica, and this should bring the theoretical value $A_v$ nearer 5.96 for them than for silica. Moreover, the values of $A_p$ given contain the thermal effect of expansion. Allowances made for these two facts bring these substances into a class with the forms of silica, and thus tend to give assurance that the silica results are not exceptional, although the data are too uncertain to add anything to the exactness of our knowledge of the variations from the theoretical curve.

Anorthite is given as a type of substances showing above 1000° a much larger increase. This increase, however, is very likely in part the effect of the latent heat from a slight amount of melting, due to a very small amount of impurity. Such effects have regularly been found in other kinds of work on silicates. But while such melting can affect the atomic heat as determined for anorthite and most other substances, which crystallize readily on cooling, it can scarcely do the same for the other silicon compounds given here, all of which are very sluggish in their changes of state, so that any slight portion of them melted at the higher temperatures would in all probability merely cool to a glass during the heat determination in the calorimeter, giving out no latent heat. Indeed, since the effect of impurities in causing melting increases rapidly with the temperature, these substances are very exceptional in their value for demonstrations at high temperatures such as that which is the subject of this paper.

*Summary.*—The specific heats of three forms of silica and two silicates (alkaline feldspars) determined for temperatures up to 1300°, indicate that the atomic heats at constant volume for these substances increase above theoretical value, asymptotic to 5.96, as the heats of metals have been known to do, and hence that such increase is a very general phenomenon, as has been suspected.

[1] A fuller account will be published elsewhere.

[2] Nernst, W., *Ann. Physik, Leipzig,* **36,** 1911, (430).

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# *ON CERTAIN PROJECTIVE GENERALIZATIONS OF METRIC THEOREMS, AND THE CURVES OF DARBOUX AND SEGRE*

By Gabriel M. Green

Department of Mathematics, Harvard University

Communicated by G. D. Birkhoff, September 18, 1918

About a year ago appeared in these Proceedings an abstract[1] of some investigations which I was then engaged in preparing for publication. As the work progressed it expanded, and I wish to devote this note to a description of some of my new results. The details of this discussion will appear with some of those of the former note, in extended form, in a single memoir on the general theory of surfaces.

Let $P$ be a regular point of a non-developable surface $S$, and $l$ a line in the tangent plane to $S$ at $P$, but not passing through $P$. In my preceding note, I defined a line $l'$ which is in the relation $R$ to the line $l$ with respect to the asymptotic net on $S$. I shall henceforth say that the lines $l$ and $l'$ are reciprocal to each other; they are, in fact, reciprocal polars of the osculating quadric[2] of the surface at $P$. The line $l'$ passes through the point $P$, and does not lie in the tangent plane to $S$ at $P$. If at each point of $S$ we construct a line $l'$ protruding from the surface, we obtain a congruence $\Gamma'$; I shall say that the congruence $\Gamma'$ and the congruence $\Gamma$, formed of the lines $l$ which are reciprocal to the lines $l'$, are reciprocal to each other.

A projective generalization of the definition of geodesics on a surface $S$ may easily be formulated, if the congruence of normals of the surface be replaced by a congruence $\Gamma'$ composed of lines $l'$ protruding from the surface and defined projectively in terms of the surface. There exists, in fact, a two-parameter family of curves on the surface, whose osculating planes contain the corresponding lines of the congruence $\Gamma'$. I shall, with Miss Pauline Sperry,[3] call these *union curves* of the congruence $\Gamma'$. In my previous note, I suggested that in a projective generalization of metric theorems it is desirable to preserve the important property possessed by the congruence of normals: that its developables cut the surface in a conjugate net. I also characterized a congruence $\Gamma'$ having this property,[4] and uniquely determined by the surface. We may call it the *pseudo-normal congruence* of the surface, and the lines of which it is composed the *pseudo-normals* of the surface. I have recently succeeded in showing that this congruence is the most suitable substitute for the congruence of normals, in attempting to generalize the surfaces of Voss. A surface of Voss is characterized by the property, that among its geodesics exist two one-parameter families which form a conjugate net; in addition to this defining property, however, is the important one, that this conjugate net of geodesics has equal tangential invariants. I have proved that this property is merely a consequence of the fact that the developables of the congruence of normals cut the surface in a conjugate net; that is, *if there exist a conjugate net formed of union curves of a congruence* $\Gamma'$, *and if this conjugate net have equal tangential invariants, then the developables of* $\Gamma'$ *must cut the surface in a conjugate net.* This theorem turns out to be a consequence of the general theory of reciprocal congruences.

The definition of union curves of a congruence $\Gamma'$ may be dualized so as to yield a new two-parameter family of curves, related to a congruence $\Gamma$, which we shall call *adjoint union curves.* These curves are defined as follows.[3] Let $C$ be any curve on the surface; then the tangents to the surface which are conjugate to the tangents of $C$ form a developable, and the edge of regression of this developable we may call the first Laplace transform of $C$. In fact, each point of this edge of regression I shall call the first Laplace transform of the corresponding point of $C$. Let $\Gamma$ be a congruence composed of lines $l$, one in each tangent plane of the surface. Then the curve $C$ is said to be an *adjoint union curve* of the congruence $\Gamma$ if the first Laplace transform of every point $P$ of $C$ lies on the corresponding line $l$ of $\Gamma$.

A study of the relations between the union curves of a congruence $\Gamma'$ and the adjoint union curves of the reciprocal congruence $\Gamma$ leads to many interesting results in the general theory of surfaces and rectilinear congruences. In particular, light is thrown on a number of theorems which first appeared in my second memoir on conjugate nets.[5] In the terminology of that paper, combined with the concepts of the present note, *if* $\Gamma'$ *be the axis congruence of a conjugate net, then the reciprocal congruence* $\Gamma$ *is the ray congruence of the associate conjugate net.* That is, the first-mentioned conjugate net is formed of union curves of the congruence $\Gamma'$, and its associate conjugate net consists of adjoint union curves of the congruence $\Gamma$ which is reciprocal to $\Gamma'$. This theorem is fundamental in the study of surfaces of Voss and their projective generalization.

In general, the union curves of a congruence $\Gamma'$ do not coincide with the adjoint union curves of the reciprocal congruence $\Gamma$; in fact, this can happen only on a quadric, and always does on such a surface. But on any non-ruled surface $S$, there are particular curves, which are union curves of certain congruences $\Gamma'$, and at the same time adjoint union curves of the reciprocal congruences $\Gamma$. These curves are very important in several connections, and the property just described serves as a simple geometric characterization of them. They were first defined, in a different way, by Segre,[6] and I shall therefore call them the *curves of Segre;* they are, however, merely curves which are conjugate to certain similar curves which were previously introduced by Darboux[7] in a memoir on the contact of curves and surfaces.

The curves of Darboux were called by him *lines of quadric osculation.* He defined them as follows. Each member of the three-parameter family of quadrics which have contact of the second order with the non-ruled surface $S$ at a regular point $P$ cuts the surface in a curve which has a triple point at $P$. The three tangents at such a triple point may coincide, but, if they do, it must be in one of three directions, which Darboux calls *tangents of quadric osculation.* Associated with each of these three tangents of quadric osculation is a one-parameter family of quadrics having second-order contact with the surface at $P$, and cutting the surface in a curve having a triple point with coincident tangents in that particular direction of quadric osculation. There are, therefore, three one-parameter families of curves, the *curves of Darboux,* whose tangents are tangents of quadric osculation. The curve of Segre may likewise be assembled into three one-parameter families, each family being conjugate to a family of curves of Darboux.

Both Segre's and Darboux's treatments depend essentially on the notion of order of contact. It has always seemed to me to be desirable, whenever possible, to supply equivalent treatments, in such cases, which are independent of this notion; not necessarily to replace the older theorems, but to afford new theorems which are frequently of more value in practice. Moreover, it is generally difficult to define order of contact in a purely geometric way, especially projectively. Consequently the following theorem is not without interest: *on any non-ruled surface S there exist three one-parameter families of curves, the members of each family of which are union curves of a congruence* $\Gamma'$ *and at the same time adjoint union curves of the reciprocal congruence* $\Gamma$. These are the curves of Segre,[6] and the curves of Darboux may be characterized as composing the three families conjugate to them.

The generalization of surfaces of Voss described above is only one of the many instances in which an important metric property of a configuration is really a particular case of a far more general projective one. I have found a number of others, connected not only with the subject of geodesics but also with apparently unrelated concepts; there appears throughout a unifying feature, however, in the notion of reciprocal congruences.

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[1] Green, G. M., these PROCEEDINGS, **3,** 1917, (587–592).

[2] Wilczynski, E. J., *Trans. Amer. Math. Soc., New York,* **9,** 1908, (79–120). Cf. top of p. 83.

[3] Sperry, P., *Amer. J. Math., Baltimore,* **40,** 1918, (213–224).

[4] Green, G. M., *loc. cit.,* pp. 590–591. The congruence referred to is that generated by the lines $y\zeta$.

[5] Green, G. M., *Amer. J. Math., Baltimore,* **38,** 1916, (287–324).

[6] Segre, C., *Ac. dei Lincei, Rend., Rome,* (Ser. 5), $\mathbf{17_2}$, 1908, (409–411).

[7] Darboux, G., *Bull. Sci. Math., Paris,* (Ser. 2), **4,** 1880, p. 356.

# THE RECTANGULAR INTERFEROMETER WITH ACHROMATIC DISPLACEMENT FRINGES IN CONNECTION WITH THE HORIZONTAL PENDULUM

By Carl Barus

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Communicated, September 16, 1918

*1. Introduction.*—In the *Reports of the Carnegie Institute of Washington,* 1915, No. 229, Chap. I, part 2, pp. 30 et seq., I adduced a method for the application of the displacement interferometer to the horizontal pendulum with a graphic exhibit of the results obtained during a series of months. The concave mirror design by which the spectrum interference ellipses were made available showed a very satisfactory performance. The attainable accuracy was such that for moderate constants in the installation of the pendulum, an inclination of $3 \times 10^{-4}$ seconds of arc should have been registered per vanishing interference fringe (ellipse), or about $10^{-3}$ seconds per $10^{-4}$ cm. of displacement of the micrometer. The inclination of the line of suspending pivots was here about 1° to the vertical. A smaller angle would have correspondingly increased the sensitiveness.

The apparatus, however, required a space about two meters long between the extreme mirrors, for its installation. This is a disadvantage, since small changes of temperature in the brackets and supports as well as in the pier would interfere with the full realization of the precision of the method. The rectangular interferometer with an auxiliary mirror is thus to be preferred; for here all the necessary parts may easily be placed within a distance of one foot from the wall of the pier carrying the horizontal pendulum. If the achromatic fringes are used, these are straight and intense, so that photographic methods are available.

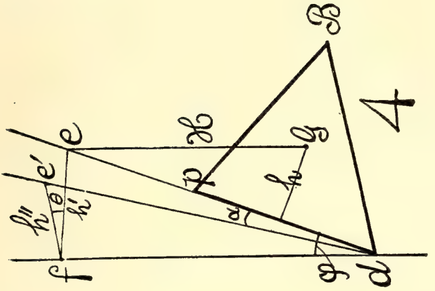
2. *Apparatus.*—The old horizontal pendulum made of thin steel tubing formerly described (l. c.) was again used.

Figure 1 gives a sectional plan of the pendulum installation, and figure 2 a front view of the pendulum *HH* alone, in a somewhat reduced scale. Its general shape is that of an isosceles triangle and the distance from the line of pivots *tt'* to apex about 110 cm., while the distance between pivots was 97 cm. These pivots *S*, *S'* are fine screws ending in hard steel points, which enter a glasshard steel socket (below) and a steel groove (above). The line *tt'* of the horizontal pendulum can thus be given any inclination to the vertical, while the rods *p*, *p'* which receive the screws *S*, *S'* may be moved normally to the wall of the pier *PP'*, inward or outward, and clamped to secure parallelism between the pier *PP'* and pendulum *HH'*. The apex *B* of the pendulum is also provided with a clamp, holding vane *D* submerged in an oil vat, *v*, for damping.

The whole pendulum is enclosed by a flat case, *CC'*, of tin plate provided with a plate glass window at *g*, through which the auxiliary mirror *m* of the interferometer may be seen. This is attached to one or two vertical tubes *hh'* of the pendulum, adjustably, so that it can be moved up or down, and rotated slightly about a vertical and a horizontal axis.

The interferometer consists essentially of the 4 plate glass mirrors, *M*, *M'*, *N*, *N'*, all but *M* being half silvers, the collimator (beyond *L*) and the telescope at *T* or *T'*, *t''* being a telescope support. The collimated white beam *L* is thus separated into the component rays *L*, *e*, *m*, *e*, *a*, *d*, *T*, and *L*, *b*, *f*, *m*, *f*, *c*, *T*, to be observed at either *T* or *T'*. *M'* is on a micrometer slide (not shown) with the screw normal to the face of the mirror. All mirrors should be capable of slight rotation about horizontal and vertical axes and the silvered faces all lie towards *m* for compensation of glass paths. The rays leaving *M'* for *T* must not only be accurately parallel but locally (visible as spots of light) nearly coincident; otherwise the fringes will be weak or invisible.

The telescope *T* is provided with an ocular micrometer (centimeter divided in tenth millimeters), standardized by aid of the sliding micrometer at *M'*. Moreover the image of the wide slit of the collimator adapted to the use of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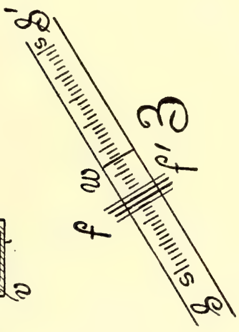
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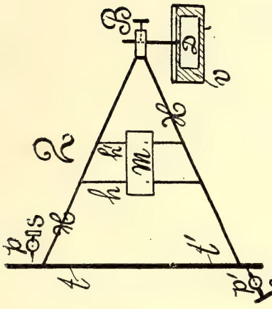
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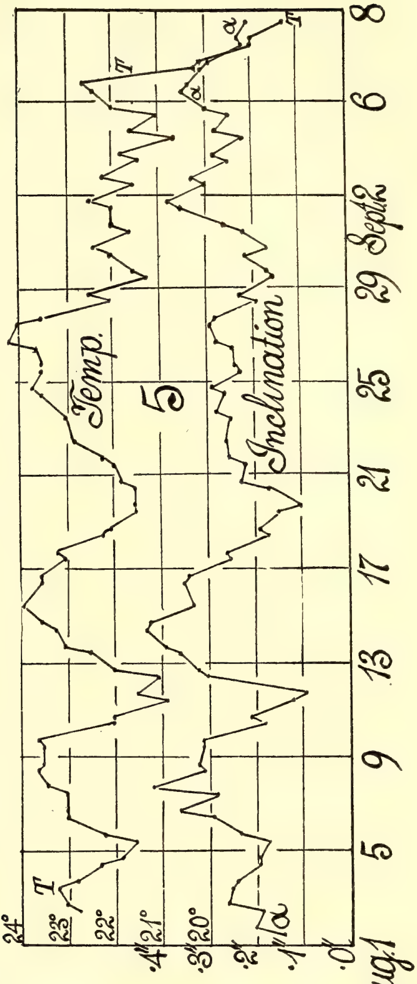
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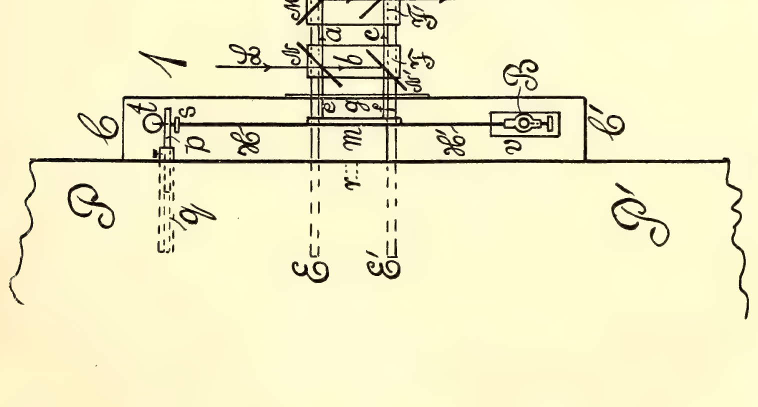
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the achromatic fringes, should be placed at right angles to them, with the ocular micrometer so placed as to read from end to end of the slit image. A very fine wire beam across the slit gives the fiducial line relative to the ocular micrometer. Figure 3 shows the general arrangement, $SS'$ being the oblique wide slit image, *ff* the achromatic fringes, *w* the image of the fiducial wire across the slit, and $ss'$ the ocular scale. Of course the fringes may be made horizontal or vertical; but this requires much adjustment (or else compensators) and is therefore an unnecessary complication of the work. With this fiducial mark at the collimator which is permanently out of reach, if the telescope is accidentally shifted, or it is temporarily removed, it may be replaced without difficulty. It is the telescope, however, which contains the ultimately fiducial scale, and it like the collimator should be held on a standard, $t''$, suitably attached to the interferometer and the pier. Similarly, the mirrors $M$ and $M'$, $N$ and $N'$ fixed in pairs to slides or carriages $F$, $F'$ are clamped to two parallel horizontal tubes $E$, $E'$ (3/8 inch gas pipe smoothed, for instance) anchored in the pier. The highest attainable rigidity in the placement of the mirrors $M$, $M'$, $N$, $N'$, and of the telescope is essential. At the outset of the work the viscous yielding of standards and braces is quite apparent.

3. *Equations.*—In figure 4 let *pBd* denote the horizontal pendulum in the plane of the diagram and *dpe* the line of pivots prolonged, terminating in *e* vertically above the center of gravity $G$. Let the inclination of *de* to the vertical be $\varphi$, a constant of the apparatus, and suppose a perpendicular $h'$ is let fall from *e* to the vertical *df* through *d*. If in consequence of a change in the inclination of the pier the line of pivots passes to $de'$, over a nearly vertical angle $\alpha$, $h'$ will pass into $h''$ over a horizontal angle $\theta$. Thus the measurement consists in finding $\alpha$ in terms of the interferometer angle $\theta$. Since these angles are all very small we may write ($\Delta$ being a differential symbol) as in the preceding paper.

$$\Delta\alpha = \varphi\Delta\theta \tag{1}$$

In the rectangular interferometer with an auxiliary mirror, if the distance apart of the rays *a* and *c*, Fig. 1, be $2R$

$$4R\Delta\theta = 2\Delta N\cos i = n\lambda \tag{2}$$

where $i = 45°$, $\Delta N$ the displacement of micrometer to bring the achromatic fringes back to the fiducial line and $n$ the number of fringes which pass that line. Hence

$$\Delta\alpha = \varphi\frac{\Delta N\cos i}{2R} = \varphi\frac{n\lambda}{4R} \tag{3}$$

The smallest angle $\Delta\alpha$ which can thus be measured depends essentially on $2R$ the breadth of the ray parallelogram. There would be no difficulty in making this as long as the line from $tt'$ to $B$ of the horizontal pendulum, i.e. over a meter;

but this would necessitate two mirrors $m$ one at each end. For the present purposes, I preferred to use apparatus which I had at hand in which $2R$ was but 10 cm. and a single mirror could be used at $m$. Nevertheless if $n = 1$, the limit of angle measurable, if $\varphi = 0.0175$ or one degree is $\Delta\alpha = 5 \times 10^{-8}$ radian per fringe; i.e. about 0.01 second of arc. With an ocular micrometer and well produced achromatic fringes there is no difficulty in estimating 1/10 fringe, so that the limiting angle here is to be a few thousandths of a second, even if $\varphi = 1°$, which may also be reduced. By making $2R = 100$ cm. one should therefore be able to reach 0.0001 second per tenth fringe breadth, if $\varphi$ is 1°.

Finally on using the ocular micrometer for moderately sized fringes of say one scale part (0.1 millimeter fringes in the ocular) the case is equally promising. A comparison of the two displacements $\Delta N$ at $M'$ and $\Delta e$ of the fringes in the ocular, showed $\Delta e/\Delta N = 265$. Hence

$$\Delta\alpha = \varphi\Delta e \cos i/265 \times 2R$$

If $\Delta e/\Delta N = 10^{-2}$ (one scale part) and $2R = 10$ cm. etc., as above,

$$\Delta\alpha = 2.7 \times 10^{-8}$$

or 0.005″ per scale part of the ocular micrometer. A few tenths of this may be estimated on the scale. The sensitiveness will be ten times greater if $2R$ is a meter.

*4. Observations.*—The interferometer was installed with rather smaller fringes than instanced above and therefore with less sensitiveness, as the inclination of the pier in a heated laboratory would probably run into seconds of arc in the lapse of time. For this reason $R$ was also satisfactory at its small value of $2R = 10$ cm. The angle $\varphi$ was directly measureable, as the inclination of the line joining the points of the pivots to the plumb line. Under these circumstances $\Delta\alpha = 0.9\ \Delta e$ seconds, roughly, the tenth millimeters of the ocular scale are about 1/100 second of arc in relation to $\Delta\alpha$ and the fringes were of about the same size. There would have been no difficulty in making them much larger and therefore more sensitive as they were clear and strong. The end of the compound pendulum was damped in lubricating oil.

The earlier observations were discarded; but even after January 14 (after which time the apparatus worked smoothly), there are instances of displacement within the apparatus requiring readjustment. These betray themselves in a lack of coincidence of the two wide slit images, Fig. 3, or of the cross wire $w$ (the slit is really superfluous and the collimator lens may be so placed as to widen the illuminated field).

Though observations were made continuously since January, I can here only give the graphs for the days of August and September, just passed. In figure 5, the lower curve shows the apparent change of inclination of the pier in seconds of arc; the upper curve the corresponding temperature (in degrees centigrade) of the basement laboratory room. Observations were usually made at 10 a.m. and 6 p.m. Between August 1 and 21, the resemblance of the two curves is marked. After that the gross resemblance is no longer so striking, but it nevertheless remains in the details and there is a mere lack of equivalence, quantitatively. The question therefore arises where this temperature discrepancy has its seat. It can hardly be in the interferometer, where the parts are of the same metal except in the (virtually) tetrahedral bracket, consisting of the rods, $E$, $E'$, and the brace downward from $t''$ to $r$ in the pier. For here one part $t''r$ is of iron and the other from $EE'$ downward to $r$, of brick. There might thus be differential expansion; but the interferometer would not be sensitive to this motion, of which I convinced myself by bearing down hard at $t''$ with the hands. No adequate displacement of fringes resulted. Hence it appears probable that what is observed is the warping of the pier, etc., as a result of the inward progress of the successive isotherms through it, beginning at the parts least protected against changes of temperature by the surrounding house walls. At all events this temperature feature is so serious that a few tenths of a degree centigrade can not be overlooked.

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PROCEEDINGS
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Volume 4 DECEMBER 15, 1918 Number 12

# THE ABSORPTION SPECTRUM OF THE NOVAE

By W. S. Adams
Mount Wilson Observatory, Carnegie Institution of Washington

Communicated, September 24, 1918

One of the most remarkable features of the spectrum of novae or temporary stars is the presence at a certain stage in their development of absorption lines displaced greatly toward the violet end of the spectrum. In the case of the elements hydrogen and helium they appear as strong lines forming sharp boundaries to the broad emission bands which are the principal characteristic of the spectrum of such stars. Occasionally they are double and are subject to great variations in character and intensity. In addition to the lines of hydrogen and helium a large number of absorption lines has been observed in the spectra of the three brightest novae of recent years, Nova Persei of 1901, Nova Geminorum of 1912, and Nova Aquilae of 1918. The identification of many of these lines and some features of their displacements form the object of this communication.

Nova Aquilae was first observed at Mount Wilson on June 8. At this time the spectrum was essentially continuous, with a few broad and hazy absorption bands superposed upon it. Most of these were due to hydrogen and helium and were displaced over 20 angstroms toward the violet. Two nights later the entire spectrum had changed: a large number of comparatively narrow absorption lines had appeared and the hydrogen and helium lines had become double. This spectrum had so many points of similarity to that of certain stars that a comparison was instituted with a photograph of $\alpha$ Cygni, a star of early type with exceptionally strong enhanced lines. It then appeared that a considerable number of lines could be identified as common to both spectra provided a displacement of about $-23$ angstroms at $\lambda$ 4500 were assumed in the case of Nova Aquilae. A large proportion of these lines are enhanced but some of the stronger arc lines are also represented. In the region of spectrum between $\lambda$ 3900 and $H\beta$ ninety lines have been identified with considerable certainty, and the origin of many others may be assigned with only slightly less probability.

A similar comparison with the lines measured in the spectrum of Nova Geminorum of 1912[1] shows clearly the presence of many of these lines in the latter star as well. The displacement, however, is only about one-half as great as in the case of Nova Aquilae. To illustrate the character of the lines identified and the displacements involved the results for a portion of the spectrum between λ 4500 and λ 4600 are given in table 1.

The lines in the spectrum of Nova Geminorum are much more hazy and ill-defined than those in Nova Aquilae and the agreement is much less satisfactory. This is due in part to the fact that the earliest photographs of the spectrum of Nova Geminorum were obtained at a longer interval after the discovery of the star than in the case of Nova Aquilae, and that during this interval the absorption lines had begun to grow faint and disappear. About forty lines common to the spectra of these two stars have been identified in the region between Hδ and Hβ.

TABLE 1

| WAVE-LENGTH | | | DISPLACEMENT | | ELEMENT |
|---|---|---|---|---|---|
| Sun | Nova Aquilae (June 12) | Nova Geminorum | Nova Aquilae A | Nova Geminorum A | |
| 4501.4 | 4478.2 | 4489.3 | −23.2 | −12.1 | Enhanced Ti |
| 4508.5 | 4485.3 | 4495.5 | 23.2 | 13.0 | Enhanced Fe |
| 4515.5 | 4492.0 | 4505.2 | 23.5 | 10.3 | Enhanced Fe |
| 4520.4 | 4497.1 | 4508.1 | 23.3 | 12.3 | Enhanced Fe |
| 4522.8 | 4499.2 | 4511.9 | 23.6 | 10.9 | Enhanced Fe |
| 4528.8 | 4505.3 | 4518.4 | 23.3 | 10.4 | Arc line Fe |
| 4534.1 | 4510.5 | 4522.8 | 23.6 | 11.3 | Enhanced Ti |
| 4549.8 | 4526.1 | 4537.6 | 23.7 | 12.2 | Enhanced Fe, Ti |
| 4556.1 | 4531.7 | 4543.8 | 24.4 | 12.3 | Enhanced Fe |
| 4558.8 | 4535.0 | 4547.0 | 23.8 | 11.8 | Enhanced Cr |
| 4563.9 | 4540.1 | 4551.6 | −23.8 | −12.3 | Enhanced Ti |

It is known from the investigations of Campbell and Wright on the spectrum of Nova Persei of 1901[2] that the displacements of the absorption lines of hydrogen were directly proportional to wave-length in this star, and the same result was found for Nova Geminorum by other observers. The identification of the metallic lines in the case of Nova Aquilae and Nova Geminorum makes a study of their displacements of especial interest in this regard. For this purpose the average displacement and the average wave-length have been formed for groups of lines distributed throughout the spectrum. In the less refrangible region longer intervals have been included because of the smaller number of lines available and the larger range of error in wave-length. The results are given in table 2.

The values for Nova Persei are by Campbell and Wright[2] and those for Nova Aurigae of 1892 are by Campbell and Vogel.[3]

These results may be represented by the following equations in which $\Delta\lambda$ is the displacement. In the case of Nova Aurigae the results are too fragmentary to allow of an accurate determination of the coefficient of $\lambda$.

| | | *Displacement at λ 4500* |
|---|---|---|
| Nova Aquilae: | $\Delta\lambda = 0.00513\,\lambda$ | −23.1 |
| Nova Persei: | $\Delta\lambda = 0.00510\,\lambda$ | −22.9 |
| Nova Geminorum: | $\Delta\lambda = 0.00257\,\lambda$ | −11.5 |
| Nova Aurigae: | $\Delta\lambda = 0.0025\,\lambda$ | −11.4 |

TABLE 2

| MEAN λ | MEAN Δλ | NUMBER OF LINES | O − C |
|---|---|---|---|
| | *Nova Aquilae* | | |
| 4020 | 20.8 | 5 | +0.2 |
| 4155 | 21.5 | 4 | +0.2 |
| 4260 | 22.0 | 7 | +0.1 |
| 4345 | 22.5 | 11 | +0.2 |
| 4450 | 22.9 | 7 | +0.1 |
| 4540 | 23.6 | 13 | +0.3 |
| 4630 | 24.0 | 2 | +0.2 |
| 4820 | 24.6 | 4 | −0.1 |
| 4990 | 25.5 | 3 | −0.1 |
| 5230 | 27.2 | 8 | +0.4 |
| 5350 | 27.6 | 8 | +0.2 |
| 5510 | 28.1 | 5 | −0.2 |
| 5670 | 27.8 | 4 | −1.3 |
| 5890 | 29.4 | 5 | −0.8 |
| 6390 | 32.7 | 10 | −0.1 |
| | *Nova Geminorum* | | |
| 4220 | 10.6 | 7 | −0.2 |
| 4350 | 10.9 | 7 | −0.3 |
| 4445 | 11.2 | 5 | −0.2 |
| 4540 | 11.8 | 13 | +0.2 |
| 4935 | 12.8 | 3 | +0.1 |
| 5896 | 15.3 | 1 | +0.2 |
| 6563 | 17.9 | 1 | +1.1 |
| | *Nova Persei (H lines)* | | |
| 3890 | 19.3 | 1 | −0.5 |
| 3970 | 20.0 | 1 | −0.2 |
| 4102 | 20.5 | 1 | −0.4 |
| 4341 | 22.2 | 1 | +0.1 |
| 4862 | 25.7 | 1 | +0.9 |
| | *Nova Aurigae (H lines)* | | |
| 4102 | 10.3 | 1 | 0.0 |
| 4341 | 11.4 | 1 | +0.5 |
| 4862 | 11.9 | 1 | −0.4 |

The differences, observed minus computed, are given under O − C in table 2. The agreement is close except in the less refrangible part of the spectrum where the determinations of wave-length are least accurate.

We find, accordingly, the surprising result that the displacements of the lines in all of these stars are directly proportional to wave-length and divide themselves into two pairs of equal amount. Of these the first pair of stars has exactly twice the displacement of the second, and it is perhaps a significant fact that Nova Aquilae and Nova Persei were much brighter stars apparently, and probably intrinsically as well, than Nova Geminorum and Nova Aurigae.

A very peculiar phenomenon in the case of Nova Aquilae is the progressive increase in the values of the displacements of the absorption lines at successive dates. Thus in the region between λ 4250 and λ 4600 we find for the dates June 10 to June 15:

| June 10 | June 11 | June 12 | June 13 | June 15 |
|---|---|---|---|---|
| −21.7(30) | −22.4(30) | −23.0(26) | −23.7(19) | −24.6(11) |

During this period the average daily increase in displacement is slightly over 0.5 angstrom. The decrease in the number of lines (given in parenthesis) shows the gradual disappearance of the absorption spectrum within this time.

Reference has already been made to the double character of the hydrogen and helium lines during the interval when the absorption spectrum of Nova Aquilae was prominent. A similar characteristic was found in the case of Nova Geminorum. The less refrangible of these components shows displacements which are in close agreement with those of the absorption lines due to iron and other elements which have been identified. An investigation of the more refrangible component shows that its displacements may also be represented as a direct function of the wave-length. We find for the two stars the relations:

Nova Aquilae: $\Delta\lambda = 0.0075\lambda$:     Nova Geminorum: $\Delta\lambda = 0.0048\lambda$

The differences as computed from these equations are as follows:

| | NOVA AQUILAE (JUNE 12) | | | NOVA GEMINORUM | | |
|---|---|---|---|---|---|---|
| | Observed | Computed | O − C | Observed | Computed | O − C |
| H............... | 30.7 | 30.8 | −0.1 | 19.4 | 19.7 | −0.3 |
| H............... | 31.7 | 32.6 | −0.9 | 20.6 | 20.8 | −0.2 |
| Hβ............... | 37.6 | 36.5 | +1.1 | 23.7 | 23.3 | +0.4 |
| Hα............... | 49.0 | 49.2 | −0.2 | | | |

At $\lambda$ 4500 we find the displacements

Nova Aquilae; −33.8     Nova Geminorum; −21.6

In the case of Nova Geminorum, therefore, the displacement of this component of the hydrogen lines is almost exactly twice that of the less refrangible component and the numerous metallic lines, while in Nova Aquilae it is one and one-half times as great. In other words 11, 22 and 33 angstroms, in the harmonic ratio 1, 2 and 3, represent very closely all the displacements found among the absorption lines in the spectra of these four stars with the exception of the four narrow nearly undisplaced lines of calcium and sodium from which the radial velocities of the stars may be derived. These lines, like the broad emission bands of hydrogen and helium, may perhaps be considered as belonging to the stars themselves, while the greatly displaced absorption lines originate in an outer envelope possibly detached from the body of the stars.

It is certain that no adequate explanation has been offered as yet to account for these immense displacements. Were the hydrogen and helium lines alone involved it is possible that some of the complex phenomena of self-reversal under conditions of marked variations in density and pressure might, at least in part, be responsible. The presence, however, of a great number of comparatively narrow single absorption lines unaccompanied by emission bands cannot be explained in this way. Their character, as well as the law of variation of displacement with wave-length, also precludes the agency of pressure (assuming that negative displacements can be produced by pressure) and the Zeeman effect. No dependence of anomalous dispersion upon wave-length is known, even if it were adequate to produce lines of such a character with such displacements.

In some respects the Doppler effect accounts most nearly for the results observed. Motion in the line of sight would produce displacements directly proportional to wave-length and might leave the spectral lines well-defined. The velocities involved, −750 km. in the case of Nova Geminorum and Nova Aurigae, and −1500 km. for Nova Aquilae and Nova Persei, though large, are of the order found for some spiral nebulae, and not many times greater than those observed in some of the solar prominences. If the more refrangible component of the hydrogen, helium and calcium lines is considered, the velocities amount to −1500 and −2200 km. respectively. These values are for motion in the line of sight. If a generalized form of the Doppler principle is used a part or the whole of the effect might be referred to a rapid change in the thickness of stratum of gas producing the absorption, or in its refractive index. This hypothesis was suggested by W. Michelson[4] to account for the high velocities observed in solar prominences, and by Paddock[5] in the case of Nova Aquilae. Some objections to this view are the rate of change in refractive index or in thickness of the gas which would be required, especially since a decrease of index would be needed to produce displacements toward shorter wave-lengths; the harmonic relationship found among the displacements; and the relatively narrow character of the absorption lines.

The last two objections might also apply to motion in the line of sight. The suggestion, however, may be made that the absorption lines are produced in a shell of gas which is moving radially outward from the star with a high velocity. If the size of this shell is large as compared with that of the body of the star it is evident that an area of the shell only equal to that of the star would be seen in projection against the latter and would give absorption lines, and that all of the gas within this area would have large components of velocity toward the observer. This would result in comparatively narrow absorption lines. The remainder of the shell of gas would give an emission spectrum, and the combination of the widely different velocities would result in very broad bright bands with their centers nearly undisplaced. This is in accordance with observations. With these high velocities the interval of two days between the outburst of the star and the appearance of the prominent absorption spectrum would be sufficient for the gas to reach a great distance from the surface of the star. The hypothesis would, however, leave unexplained the apparent acceleration of motion during the period of observation of the absorption spectrum; and the nearly constant character of the emission bands after the disappearance of the absorption spectrum would point rather to their origin in the star itself.

The possibility of some form of dissociation is suggested by the harmonic relationship between the displacements of the components of the hydrogen lines, and the early appearance of the nebular lines in the spectra of these stars adds interest to considerations of this nature.

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[1] Adams, W. S., and Kohlschütter, A., *Mt. Wilson Contr.* No. 62, *Astroph. J., Chicago, Ill.*, **36,** 1912, (293–321).

[2] Campbell, W. W., and Wright, W. H., *Lick Obs. Bul.* No. 8, *Berkeley, Cal.*, 1901.

[3] Scheiner, J., *Astronomical Spectroscopy* (Frost), *Boston, Mass.*, 1894, p. 290.

[4] Michelson, W., *Astroph. J., Chicago, Ill.*, **13,** 1901, (192–198).

[5] Paddock, G. F., *Pub. Astr. Soc. Pac., San Francisco, Cal.*, **30,** 1918, (244–249), p. 249.

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# ON JACOBI'S EXTENSION OF THE CONTINUED FRACTION ALGORITHM

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It has been known since Lagrange[1] that the regular continued fraction which represents a quadratic surd becomes periodic after a finite number of non-periodic partial quotients, and conversely, a regular continued fraction which becomes periodic after a finite number of non-periodic partial quotients is one root of a quadratic equation with rational coefficients. It is useless, therefore, to look for periodicity in regular continued fractions which represent cubic and higher irrationalities. To meet this difficulty Jacobi[2] undertook to extend the continued fraction algorithm as follows:

In the case of the ordinary continued fraction we are concerned with two series of numbers, $A_n$, $B_n$, (the numerators and denominators of the successive convergents) which are given by the recursion formulae

$$A_n = q_n A_{n-1} + A_{n-2}, \quad B_n = q_n B_{n-1} + B_{n-2},$$

with the initial values $\mathrm{A}_0 = 0$, $A_{-1} = 1$, $B_0 = 1$, $B_{-1} = 0$. Jacobi considers three series of numbers, $A_n$, $B_n$, $C_n$, which are given by the recursion formulae

$$\begin{aligned} A_n &= p_n A_{n-1} + q_n A_{n-2} + A_{n-3}, \\ B_n &= p_n B_{n-1} + q_n B_{n-2} + B_{n-3}, \\ C_n &= p_n C_{n-1} + q_n C_{n-2} + C_{n-3}, \end{aligned}$$

with the initial values:

$$(A_{-2}, A_{-1}, A_0) = (1, 0, 0), \quad (B_{-2}, B_{-1}, B) = (0, 1, 0),$$
$$(C_{-2}, C_{-1}, C_0) = (0, 0, 1),$$

Jacobi then chose the coefficients of the expansion, $p_n$, $q_n$, so that $A_n : B_n : C_n$ should approximate $1 : \theta : a + b\theta + c\theta^2$ where $\theta$ is the real root of a cubic equation, and showed that these coefficients sometimes become periodic. Bachmann[3] showed later that with the coefficients determined by Jacobi's method periodicity will not always ensue. Berwick[4] has obtained periodic expansion for cubic irrationalities but his algorithm differs from Jacobi's.

A closer study of Jacobi's expansion reveals many remarkable points of contact with ordinary continued fractions not hitherto observed. Instead of starting with a cubic irrationality and finding an expansion to fit it, we start with a periodic expansion and find associated with it a definite cubic irrationality.

The set of three numbers $(A_n, B_n, C_n)$ defined as above we call the nth convergent to the ternary continued fraction,

$$(p_1, q_1;\ p_2, q_2;\ p_3; q_3; \ldots\ldots\ldots)$$

The extension to four or more sets is obvious, and most of the theorems which follow hold also for quaternary and higher continued fractions. We reserve discussion of these, however.

The follwing theorem is of fundamental importance:

*Theorem I: If* $(A_\lambda, B_\lambda, C_\lambda)$ *is the convergent of order* $\lambda$ *in the ternary continued fraction* $(p_1, q_1; p_2, q_2, \ldots.)$, *and* $(A'_{\lambda'}, B'_{\lambda'}\ C'_{\lambda'})$ *the convergent of order* $\lambda'$ *in the ternary continued fraction* $(p'_1, q'_1;\ p'_2, q'_2; \ldots..)$ *then the convergent of order* $\lambda + \lambda'$, $(A''_{\lambda + \lambda'}, B''_{\lambda + \lambda'}, C''_{\lambda + \lambda'})$, *of the ternary continued fraction* $(p_1, q_1; p_2, q_2; \ldots; p_\lambda, q_\lambda;\ p'_1, q'_1;\ p'_2, q'_2; \ldots;\ p'_{\lambda'}, q'_{\lambda'})$ *may be obtained by the formulae.*

$$\begin{aligned} A''_{\lambda + \lambda'} &= A_\lambda C'_{\lambda'} + A_{\lambda - 1} B'_\lambda + A_{\lambda - 2} A'_{\lambda'} \\ B''_{\lambda + \lambda'} &= B_\lambda C'_{\lambda'} + B_{\lambda - 1} B'_{\lambda'} + B_{\lambda - 2} A'_{\lambda'}, \\ C''_{\lambda + \lambda'} &= C_\lambda C_{\lambda'} + C_{\lambda - 1} B'_{\lambda'} + C_{\lambda - 2} A'_{\lambda'}. \end{aligned}$$

More generally, the convergents of order $\lambda + \lambda'$, $\lambda + \lambda' - 1$ and $\lambda - \lambda' - 2$ may be obtained by the rule for the multiplication of determinants, so that

$$\begin{vmatrix} A_\lambda, & A_{\lambda - 1}, & A_{\lambda - 2} \\ B_\lambda, & B_{\lambda - 1}, & A_{\lambda - 2} \\ C_\lambda, & C_{\lambda - 1}, & C_{\lambda - 2}, \end{vmatrix} \cdot \begin{vmatrix} C'_{\lambda'}, & C'_{\lambda - 2}, & C'_{\lambda' - 2} \\ B'_{\lambda'}, & B'_{\lambda' - 1}, & B'_{\lambda' - 2} \\ A'_{\lambda'}, & A'_{\lambda' - 1}, & A'_{\lambda' - 2} \end{vmatrix} = \begin{vmatrix} A''_{\lambda + \lambda'}, & A''_{\lambda + \lambda' - 1}, & A''_{\lambda + \lambda' - 2} \\ B''_{\lambda + \lambda'}, & B''_{\lambda + \lambda' - 1}, & B''_{\lambda + \lambda' - 2} \\ C''_{\lambda + \lambda'}, & C''_{\lambda + \lambda' - 1}, & C''_{\lambda + \lambda' - 2} \end{vmatrix}$$

The proof is by induction.

Consider now the purely periodic ternary continued fraction of period $k$:

$$\overline{(p_1, q_1;\ p_2, q_2;\ \ldots\ldots\ldots\ldots p_k, q_k)}$$

Associated with this fraction is the following cubic equation which we shall call the *characteristic equation* of the ternary continued fraction:

$$\begin{vmatrix} A_{k - 2} - \rho, & B_{k - 2} & C_{k - 2} \\ A_{k - 1}, & B_{k - 1} - \rho, & C_{k - 1} \\ A_k, & B_k & C_k - \rho \end{vmatrix} = 0$$

Written at length this is

$$\rho^3 - M\rho^2 + N\rho - 1 = 0$$

where

$$M = A_{k-2} + B_{k-1} + C_k$$

and

$$N = A_{k-2}B_{k-1} - A_{k-1}B_{k-2} + B_{k-1}C_k - B_kC_{k-1} + A_{k-2}C_k - A_kC_{k-2}.$$

We have proved the following theorem concerning this equation:

*Theorem II: The characteristic cubic of any periodic ternary continued fraction remains unaltered by any cyclic substitution of the partial quotients.*

The proof is again by induction. Using this theorem we may prove the following recursion formulae:

*Theorem III: For all integer values of* $n > 3k - 1$ *we have*

$$\begin{aligned} A_n &= MA_{n-k} - NA_{n-2k} + A_{n-3k}, \\ B_n &= MB_{n-k} - NB_{n-2k} + B_{n-3k}, \\ C_n &= MC_{n-k} - NC_{n-2k} + C_{n-3k}. \end{aligned}$$

From Theorem III we see that the $A$'s, $B$'s and $C$'s are solutions of the following linear difference equation with constant coefficients:

$$u_{x-3k} - Mu_{x-2k} + Nu_{x-k} - u_x = 0.$$

The theory of such equations is well understood (Boole's Finite Differences, p. 208). By referring to that theory we may write,

$$A_n = \Sigma K_i x_i^n \quad (i = 1, 2, 3, \ldots . 3k)$$

where $K_1, K_2, \ldots . K_3$ are independent of $n$ and $x_1, x_2, \ldots . x_n$ are the roots of

$$x^{3k} - Mx^{2k} + Nx^k - 1 = 0,$$

and so are the *kth* roots of the roots of the characteristic cubic.
This leads to the equation.

$$A_n = \rho_1^{n/k}\Sigma P_\nu \omega^{\nu n} + \rho_2^{n/k}\Sigma Q_\nu \omega^{\nu n} + \rho_3^{n/k}\Sigma R_\nu \omega^{\nu n}, \quad \nu = 1,2,3,\ldots k, \quad \omega^k = 1,$$

where $P_\nu$, $Q_\nu$, $R_\nu$, are independent of $n$, and $\rho_1$, $\rho_2$. $\rho_3$, are the roots of the characteristic cubic. Similar equations hold for $B_n$ and $C_n$

From this last result we obtain the remarkable theorem:

*Theorem IV: If the characteristic cubic has one root,* $\rho_1$, *whose modulus is greater than the modulus of either of the other two roots then*

$$\lim_{M \to \infty} (A_{n+k}/A_n) = \lim_{M \to \infty} (B_{n+k}/B_n) = \lim_{M \to \infty} (C_{n+k}/C_n) = \rho_1$$

*If the characteristic cubic has two imaginary roots whose common modulus is greater than the absolute value of the real root then the fractions* $A_{n+k}/A_n$, $B_{n+k}/B_n$, $C_{n+k}/C_n$ *do not approach any limit as n increases beyond limit.*

The characteristic cubic was obtained from the last three convergents at the end of the first period. If we form a cubic in the same way from the last three convergents at the end of the second period we get what we shall call the *second characteristic cubic.* Similarly for those of higher order. The following theorem holds:

*Theorem. V. The roots of the characteristic cubic of order* $\lambda$ *are the* $\lambda$*th powers of the roots of the first characteristic cubic.*

The proof is again by induction.

By a process similar to that by which Theorem IV was obtained we have also derived the following:

*Theorem: VI. If the characteristic cubic has one root* $\rho_1$ *whose modulus is greater than the modulus of either of the other two then the fractions* $B_n/A_n, C_n/A_n$ *and* $C_nB_n$ *approach for* $n \to \infty$ *limits which are cubic irrationalities and are connected with the irrational number* $\rho_1$ *by linear fractional transformations. If the characteristic cubic has two imaginary roots whose common modulus is greater than the absolute value of the real root then these fractions do not approach any limit.*

The actual equations connecting $\rho_1$ with $\sigma_1 = Lim\ (B_n/A_n)$ and $\sigma_2 = Lim\ (C_n/A_n)$ and $\sigma_3 = Lim\ (C_n/B_n)$, when these limits exist are a little difficult to obtain, but turn out as follows:

$$\begin{aligned}
\sigma_1 &= (B_k\rho_1 + A_kB_{k-2} - A_{k-2}B_k)/(A_k\rho_1 + A_{k-1}B_k - A_kB_{k-1})\\
\sigma_2 &= (C_{k-1}\rho_1 + C_{k-2}A_{k-1} - C_{k-1}A_{k-2})/(A_{k-1}\rho_1 + A_kC_{k-1} - A_{k-1}C_k);\\
\sigma_3 &= (C_{k-2}\rho_1 + C_{k-1}B_{k-2} - B_{k-1}C_{k-2})/(B_{k-2}\rho_1 + B_kC_{k-2} - B_{k-2}C_k).
\end{aligned}$$

The cubic equations satisfied by $\sigma_1$, $\sigma_2$ and $\sigma_3$ may be obtained from these relations, but the algebraic work involved is tedious. It turns out that all the coefficients of the cubic in $\sigma_1$ are divisible by the determinant of the transformation connecting it with $\sigma_1$. It follows that the discriminant of the cubic in $\sigma_1$ is equal to the discriminant of the characteristic cubic multiplied by the square of the determinant of the transformation. Similar results hold for $\sigma_2$ and $\sigma_3$. The actual cubic equation satisfied by $\sigma_1$ is

$$E\sigma_1^3 + F\sigma_1^2 + G\sigma_1 + H = 0$$

where, $E.\ F.\ G.\ H$ have the following values (we write for shortness $A$, $B$, $C$, for $A_k, B_k, C_k$; $A_1, B_1$. $C_1$ for $A_{k-1}, B_{k-1}, C_{k-1}$; and $A_2, B_2, C_2$ for $A_{k-2}, B_{k-2}, C_{k-2}$):

$$\begin{aligned}
E &= A^2C_1 + AA_1B_1 - AA_1C - A_1^2B,\\
F &= A_1BB_1 + A_1BC - 2A_1A_2B + AA_1B_2 + AA_2B_1 - AA_2C - AB_1^2 + AB_1C\\
&\qquad - 2ABC_1 + A^2C_2,\\
G &= -A_2^2B + A_2BB_1 + A_2BC + A_1BB_2 - 2ABC_2 - BB_1C + B^2C_1 + AA_2B_2\\
&\qquad - 2AB_1B_2 + AB_2C,\\
H &= A_2BB_2 - BB_2C + B^2C_2 - AB_2^2.
\end{aligned}$$

(The equation has been freed from the factor $A_1B^2 - AB\ (A_2 - B_1) - A^2B_2$ which is the determinant of the substitution connecting $\sigma_1$ and $\rho_1$)

Theorems V and VI hold word for word when the ternary continued fraction has a finite number of non-periodic partial quotients.

Some progress has been made in the problem of finding a periodic ternary continued fraction which shall be the development of a given cubic irrationality, but the results are not yet in final form.

[1] Lagrange, *Mem. Berlin,* **24,** 1770 *Oev.* II, **74.**

[2] Jacobi, *Ges. Werke,* VI, 385–426.

[3] Bachmann, *Crelle,* **75,** 1873, (25–34).

[4] Berwick, *Proc. London Math. Soc.,* (Ser. 2), **12,** 1913.

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# A CHARACTERIZATION OF JORDAN REGIONS BY PROPERTIES HAVING NO REFERENCE TO THEIR BOUNDARIES

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Schoenflies[1] has formulated a set of conditions under which the common boundary of two domains will be a simple closed curve. A different set has been given by J. R. Kline.[2] Carathéodory[3] has obtained conditions under which the boundary of a single domain will be such a curve. In each of these treatments, however, conditions are imposed 1) on the boundary itself, 2) regarding the relation of the boundary to the domain or domains in question. In the present paper I propose to establish the following theorem in which *all the conditions imposed are on the domain R alone.*

Theorem. *In order that a simply[4] connected, limited, two-dimensional domain R should have a simple closed curve as its boundary it is necessary and sufficient that R should be uniformly connected im kleinen.*[5]

*Proof.* Suppose the simply connected domain $R$ is connected im kleinen. Let $M$ denote the boundary of $R$, that is to say the set of all those limit points of $R$ that do not belong to $R$.

I will first show that $M$ can not contain two arcs that have in common only one point, that point being an endpoint of only one of them. Suppose it does contain two such arcs $EFG$ and $FK$ with no point in common except $F$. Let $\alpha$ and $\beta$ denote circles with common center at $F$, and with radii $r_1$ and $r_2$, respectively, such that $r_1 > r_2$ and such that $E$, $K$ and $G$ lie without $\alpha$. By hypothesis there exists a positive number $\delta_{r_2}$ such that if $X$ and $Y$ are two points of $R$ at a distance apart less than $\delta_{r_2}$ then $X$ and $Y$ lie together in a connected subset of $R$ that lies wholly within some circle of radius $r_2$. Let $\gamma$ denote a circle with center at $F$ and with a radius less than one half of $\delta_{r_2}$ and also less than $r_1 - r_2$. It is clear that if two points of $R$ are both within $\gamma$ then they lie together in a connected subset of $R$ that lies wholly within $\alpha$. There exist (fig. 1), within $\gamma$, points $E'$, $G'$ and $K'$ and arcs $E'K'$ and $K'G'$ such that 1) $E'$ and $G'$ are on the arc $EFG$ in the order $EE'FG'G$ and $K'$ is on the arc $FK$, 2) the interval $E'G'$ of the arc $EFG$ lies wholly within $\gamma$, 3) $E'K'$ has in common with $EFG$ only the point $E'$ and, in common with $FK$, only the point $K'$, 4) $K'G'$ has in common with $EFG$ only the point $G'$ and, in common with $FK$, only the point $K'$, 5) the segment $FK'$ of the arc $FK$ lies wholly within the closed curve $J$ formed by the arcs $E'K'$ and $K'G'$ and the interval $E'FG'$ of the arc $EFG$. The arc $E'K'$ together with the interval $E'F$ of $EFG$ and the interval $FK'$ of $FK$ form a closed curve $J_1$. The arc $K'G'$ together with the interval $FG'$ of $EFG$ and the interval $FK'$ of $FK$ form a closed curve $J_2$. Let $I$, $I_1$ and $I_2$ denote the interiors of $J$, $J_1$ and $J_2$ respectively. Then $I$ is the sum of $I_1$, $I_2$ and the segment $FK'$ of the arc $FK$. Let $P$, $P_1$ and $P_2$ denote points on the segments $FK'$, $E'F$ and $FG'$ respectively. The point $P$ is a boundary point of $R$ and the region $I$ contains $P$. Hence there must be points of $R$ in $I$ and therefore in $I_1$ or in $I_2$. The point $P_1$ also is on the boundary of $R$. It follows that there exist, within the circle $\gamma$, points of $R$ lying either without $J$ (and therefore without both $J_1$ and $J_2$) or within $J_1$. Similarly either $I_2$ or the exterior of $J$ contains points of $R$ that are within $\gamma$. Thus either both $I_1$ and $I_2$ contain points of $R$ or only one of them contains points of $R$ in which latter cases there must exist points of $R$ that are within $\gamma$ and without $J$. But if a connected point-set lies entirely within $\alpha$ and contains either 1) a point of $I_1$ and a point of $I_2$ or 2) a point of $I_1$ or $I_2$ and a point without $J$; then it must clearly contain at least one point of $EFG$ or of $FK$. Therefore it can not be a subset of $R$. Thus the supposition that $M$ contains two arcs $EFG$ and $FK$ with only the point $F$ in common leads to a contradiction.

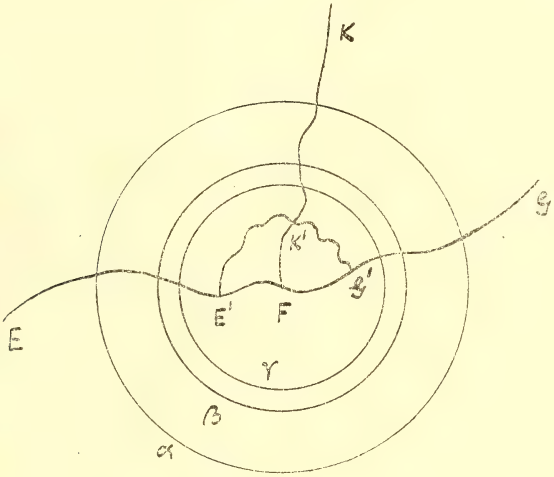


FIG. 1

That $M$, the boundary of $R$, is connected is a consequence of a theorem of Hausdorff's.[6] I will proceed to show that it is connected im kleinen. Suppose that it is not. Then there exists an infinite sequence $P, P_1, P_2, P_3, \dots$ of distinct points belonging to $M$ and a sequence of positive numbers $e, e_1, e_2, e_3, \dots$ such that 1) $e/2 > e_1 > e_2 > e_3 > \dots$, 2) $\text{Lim.}_{n=\infty}\, e_n = 0$, 3) the distance from $P_n$ to $P$ is equal to $e_n$, 4) if $n$ is a positive integer there exists no connected sub-set of $M$ containing $P_n$ and $P$ such that all the points of this sub-set are at a distance of less than $e$ from $P$. Let K denote a circle with center at $P$ and radius $e/2$. The point-set $M$ contains a closed connected subset $t_n$ that contains $P_n$ and at least one point on $K$ but no point without $K$. There does not exist an infinite sequence of distinct positive integers $\bar{n}_1, \bar{n}_2, \bar{n}_3, \dots$ such that, for every $m$, $t_{\bar{n}_m}$ has a point in common with $t_1$. For if such were the case then $P + t_1 + t_{\bar{n}_1} + t_{\bar{n}_2} + \dots$ would be a connected point-set containing $P_1$ and $P$ and lying entirely within a circle with center at $P$ and radius $e$. It follows that there exists a positive integer $n_1$ greater than 1 such that if $n \geqq n_1$ then $t_n$ has no point in common with $t_1$. Similarly there exists $n_2$ greater than $n_1$ such that if $n \geqq n_2$ then $t_n$ has no point in common with $t_{n_1}$. This process may be continued. It follows that $M$ contains an infinite sequence of closed connected point-sets $t_{n_1}, t_{n_2}, t_{n_3}, \dots$ $(n_1 < n_2 < n_3 < \dots)$ such that no two of them have a point in common and such that, for every $m, t_{n_m}$ contains $P_{n_m}$ and at least one point on $K$ but no point without $K$. Let $\bar{K}$ denote the circle whose center is $P$ and whose radius is $e_1$. For every $m$, $t_{n_m}$ contains a closed, connected subset $\bar{t}_m$ such that 1) every point of $\bar{t}_m$ is either on $K$ or $\bar{K}$ or between $K$ and $\bar{K}$, 2) $\bar{t}_m$ contains at least one point on $K$ and at least one point on $\bar{K}$. Let $K^*$ denote a circle concentric with $K$ and $\bar{K}$ and lying between them. There exists on $K^*$ a point $O$ and a sequence of points $O_1, O_2, O_3, \dots$ such that $O_m$ belongs to $\bar{t}_m$ and such that $O$ is a limit point of the point-set $O_1 + O_2 + O_3 + \dots$. Let $K'$ denote a circle lying between $K$ and $\bar{K}$ and with center at $O$. Let $2h$ denote its radius. Let $K''$ denote a circle with the same center and with a radius less than $h$ and also less than $\delta_h/2$. There exist within $K''$ four points $O_{m_1}, O_{m_2}, O_{m_3}$ and $O_{m_4}$ of the set $O_1, O_2, O_3, \dots$. Of these four points, two, $A_1$ and $A_3$, are separated from each other on $K^*$ by the other two, $A_2$ and $A_4$. For each $i$ $(1 \leqq i \leqq 4)$ let $a_i$ denote that one of the point-sets $t_{m_1}, t_{m_2}, t_{m_3}$ and $t_{m_4}$ to which the point $A_i$ belongs. For each $i$ $(1 \leqq \mathrm{i} \leqq 4)$ there exists, within $K''$ (fig. 2), a circle $K_i$, with center at $A_i$, that neither encloses nor contains a point of any of the sets $a_1, a_2, a_3, a_4$, except $a_i$. The circles $K_1$ and $K_3$ respectively enclose points $B_1$ and $B_3$ belonging to $R$. Every closed connected point-set that contains both $B_1$ and $B_3$ and lies between $K$ and $\bar{K}$ must contain a point either of $a_2$ or of $a_4$. But there exists a connected subset of $R$ that contains both $B_1$ and $B_3$ and lies wholly with $K'$. Thus the supposition that $M$ is not connected im kleinen has led to a contradiction.

Since $M$ is limited, closed, connected and connected im kleinen, it follows[7] that every two points of $M$ can be joined by a simple continuous arc lying wholly in $M$. The point-set $M$ contains a countable subset $N$ of points

$P_1$, $P_2$, $P_3$, . . . such that every point of $M$ is a limit point of $N$. For each $n$, $P_n$ can be joined to $P_{n+1}$ by a simple continuous arc $P_nP_{n+1}$ lying entirely in $M$. In view of the fact that if $M$ contains two arcs with only one point in common, that point must be an end-point of each of them, it is clear that, for every $n$, $P_1P_2 + P_2P_3 + \ . \ . \ . + P_{n-1n}P$ is either a simple closed curve or a simple continuous arc. It is clear that if, for some $n$, it is a simple closed curve, then this curve is identical with $M$. Suppose on the other hand, that, for every $n$, it is a simple continuous arc $A_nB_n$, the notation being so assigned that, in the order from $A_n$ to $B_n$ on the arc $A_nB_n$, every $A_i$ precedes every $B_j$ $(1 \leqq i \leqq n, 1 \leqq j \leqq n)$, Let $N^*$ denote the point-set constituted by the sum of all the arcs $A_1B_1$, $A_2B_2$, $A_3B_3$, . . . If $P_1$, $P_2$, $P_3$, . . . is a set of points such that for every $n$, $P_n$ follows $B_n$ on some arc of the set $A_1B_1$, $A_2B_2$, . . . then the set $P_1$, $P_2$, $P_3$, . . . does not have more than one limit point. For suppose there exists such a set with two distinct limit points $O_1$ and $O_2$. Then if $C_1$ and $C_2$ are two distinct circles with center at $O_1$ and such that $O_2$ is without each of them, there clearly exists an infinite set of arcs $G$ (Fig. 3) such that 1) each arc of $G$ is, for some $n$, $i$ and $j$ $(1 \leqq i \leqq n, 1 \leqq j \leqq n)$ a sub-interval of the interval $B_iB_j$ of the arc $A_nB_n$, 2) no two arcs of $G$ have a point in common, 3) each arc of $G$ lies entirely between $C_1$ and $C_2$ except that one of its endpoints is on $C_1$ and the other one is on $C_2$. But, in the course of the above proof that $M$ is connected im kleinen, it was shown that $M$ does not contain such a set of arcs.

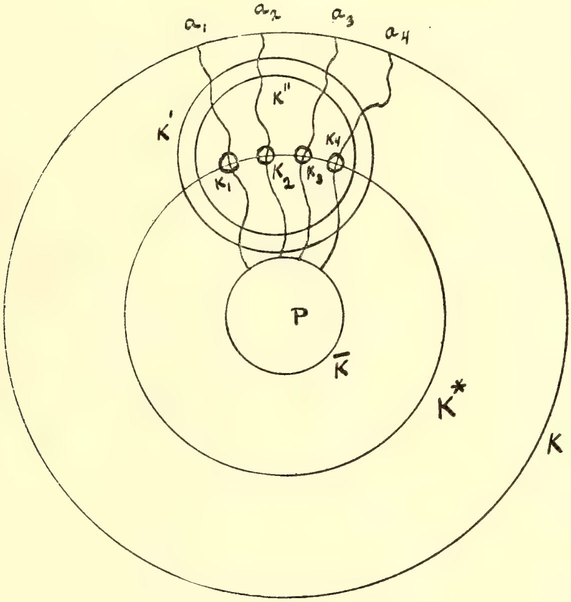
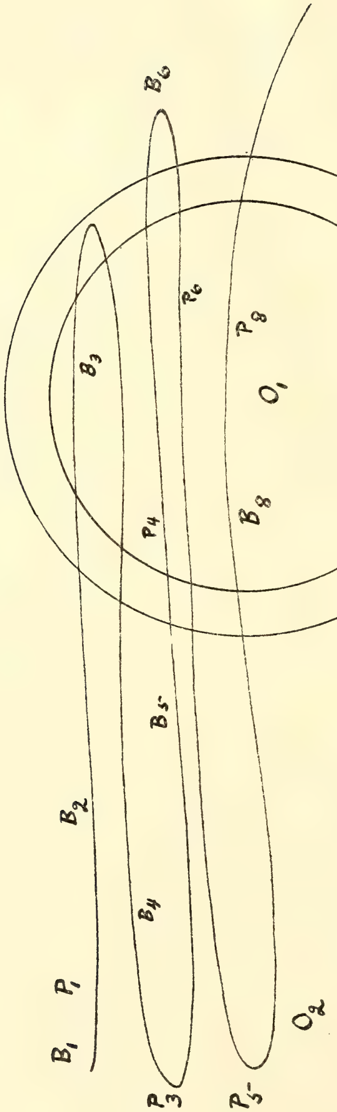


FIG.



$B_1$
$P_1$
$B_2$
$B_3$
$B_4$
$B_5$
$B_6$
$P_3$
$P_4$
$P_5$
$P_6$
$P_8$
$B_8$
$O_1$
$O_2$

FIG. 3

Similarly if $P_1, P_2, P_3, \ldots$ is a set of points such that, for every $n$, $P_n$ precedes $A_n$ on some arc of the set $A_1B_1, A_2B_2, \ldots$, then the set $P_1, P_2, P_3, \ldots$ can not have more than one limit point. It is clear that at least one of the sets $A_1, A_2, A_3, \ldots, B_1, B_2, B_3, \ldots$ is infinite. Otherwise $N^*$, and therefore $M$, would be a simple continuous arc, which is impossible in view of the fact that it is the boundary of a limited domain. There remain two conceivable cases. Either 1) the sets $A_1, A_2, A_3, \ldots$ and $B_1, B_2, B_3, \ldots$ are both infinite or 2) one is finite and the other infinite. Suppose they are both infinite. Consider the facts that 1) no set with the properties indicated above for $P_1, P_2, P_3, \ldots$ has more than one limit point, 2) no simple continuous arc is the boundary of a limited domain and, 3) $M$ does not contain two arcs with only one common point, that point being an end-point of only one of them. In view of these facts it is clear that $A_1, A_2, A_3, \ldots$ and $B_1, B_2, B_3, \ldots$ have a common limit point $O$ and that the point-set $N^* + O$ is a simple closed curve, identical with $M$.

Suppose the set $A_1, A_2, A_3, \ldots$ is infinite while $B_1, B_2, B_3, \ldots$ is not. With the aid of the same three considerations, it is clear that in this case $A_1, A_2, A_3, \ldots$ has as its only limit point the point $B_m$ where $m$ is a positive integer such that, for every $n$ greater than $m$, $B_n$ coincides with $B_m$. In this case the pount-set $N^*$ is a simple closed curve, identical with $M$. Of course a similar argument applies in case $A_1, A_2, A_3, \ldots$ is finite and $B_1, B_2, B_3, \ldots$ is infinite. Thus in every case $M$ is a simple closed curve.

I will now proceed to establish the converse proposition that *every Jordan region is uniformly connected im kleinen.* Suppose that, on the contrary, there exists a closed curve $J$ whose interior $I$ does not have this property. Then there must evidently exist a positive number $a$, a point $O$ on $J$ and two infinite sequences of points $X_1, X_2, X_3, \ldots Y_2, Y_2, Y_3, \ldots$ lying in $I$, such that $O$ is the sequential limit point of each of these sequences and such that for no $n$ can $X_n$ be joined to $Y_n$ by a connected subset of $I$ that lies entirely within a circle $K$ with center at $O$ and radius $a$. But there exists[8] a closed curve $J^*$ containing $O$ such that every point of $J^*$ belongs either to $J$ or to $K$ and such that every point within $J^*$ is within both $J$ and $K$. It is clear that for some $n$ the points $X_n$ and $Y_n$ are both within $J^*$. But the interior of $J^*$ is a connected subset of $I$ and also of the interior of $K$. Thus the supposition that $I$ is not uniformly connected im kleinen has led to a contradiction.

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[1] Schoenflies, A., *Göttingen, Nachr. Ges. Wiss.*, 1902, (185).

[2] Kline, J. R., *Bull. Amer. Math. Soc., New York*, 24, 1918, (471). One of Kline's conditions is that the common boundary of the domains in question should be connected 'im kleinen.'

[3] Carathéodory, C., *Math. Ann., Leipzig*, 73, 1912–13, (366).

[4] In space of two dimensions, a domain is said to be *simply connected* if it is connected and contains every limited point-set whose boundary lies in it.

[5] A point-set $M$ is said to be *connected 'im kleinen'* if for every point $P$ of $M$ and every positive number $e$ there exists a positive number $\delta_{ep}$ such that if $X$ is a point of $M$ at a dis-

tance less than $\delta_{ep}$ from the point $P$, then $X$ and $P$ lie together in a connected sub-set of $M$ every point of which is at a distance of less than $e$ from the point $P$. The set $M$ is said to be *uniformly* connected im kleinen if for every positive number $e$ there exists a positive number $\delta_e$ such that if $P_1$ and $P_2$ are two points of $M$ at a distance apart less than $\delta_e$ then they lie together in a connected sub-set of $M$ every point of which is at a distance of less than $e$ from $P_1$. Cf. Hahn, H., *Jahresber. D. Math. Ver., Leipzig*, **23**, 1914, (318–322).

[6] Hausdorff, F., *Grundzüge der Mengenlehre,* Veit & Co., Leipzig, 1914.

[7] Cf. my paper, A theorem concerning continuous curves, *Bull. Amer. Math. Soc.*, **23**, 1917, (233–236).

[8] Cf. Theorem 43 of my paper, On the foundations of plane analysis situs, *Trans. Amer. Math. Soc., New York*, **17**, 1916, (131–164). I take this opportunity to correct an error in the statement of Theorem 44 of this paper. In this statement the upper 'interior' and the two upper 'without's are to be omitted.

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# *A BIOMETRIC STUDY OF HUMAN BASAL METABOLISM*

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Carnegie Institution of Washington

Communicated October 8, 1918

Investigators are now generally agreed that the metabolism, expressed in terms of calories per unit of time, of the normal subject shall be taken as a basis of comparison in the investigation of all the special problems of human nutrition, for example, that of the requirements for muscular activity, that of the influence of specific diseases or of the level of nutrition upon metabolism, that of the change of metabolic activity with age, and so forth. Critical investigations in both European and American laboratories have shown that the gaseous metabolism is so affected by various factors that determinations which are to serve as a standard must be made under very exactly controlled conditions. It is not merely necessary to devise apparatus in which the physical difficulties of direct calorimetry (or of the exact measurement of gaseous exchange from which heat production may be computed) are overcome. Certain biological factors must be ruled out. Those of greatest importance as sources of experimental error are muscular activity and the stimulatory action of recently ingested food. The heat production of the individual in a state of complete muscular repose 12–14 hours after the last meal, i.e., in the postabsorptive condition, has been called the basal metabolism.

For a decade the Nutrition Laboratory has been engaged in carrying out a series of determinations of basal metabolism in normal human individuals of both sexes and of widely different ages. These have been made with all the modern refinements of method and manipulation. The subjects were in presumably good health. All those with febrile temperature were discarded. All were in the postabsorptive condition. Perfect muscular repose during the short periods required for indirect calorimetry was assured by an automatic record of all movements, even those imperceptible to a trained observer.

Measurements on 136 men, 103 women and 94 new-born infants have been analyzed biometrically with the purpose of determining the statistical constants (means, standard deviations, coefficients of variation, coefficients of correlation, and regression equations) which may serve as standard constants in work on human metabolism until those based on more extensive series of data are available. In carrying out this analysis we have proceeded on the conviction that the widest possible usefulness of laboratory investigations of human metabolism will result from basing measurements upon individuals who are in presumably good health, but who are otherwise typical of the population at large. It is only when the subjects used for experimentation are representative of the general population in type, variability and correlation that results of laboratory research upon limited series of individuals may be safely generalized for rationing or for other practical social applications. Statistical tests of the suitability of the series used in the present investigation have been applied.

The relationship between certain of the physical and physiological measurements of the individual and between the various physiological measurements has been determined. Our series of data show practically no relationship between basal or minimum pulse rate and stature or body weight in adults. There is a low but significant positive correlation between minimum or basal pulse rate and gaseous exchange and heat production. The Nutrition Laboratory has long emphasized the correlation between pulse rate and metabolism in the same individual, that is, the intra-individual correlation between the rate of the heart beat and the amount of the katabolism. Here, however, we are dealing with the problem of the relationship between the minimum or basal pulse rate of a series of individuals and their basal metabolism—that is, with inter-individual correlation.

There is a substantial correlation between stature and heat production. The correlation between body weight and heat production is higher being of the order $r = 0.75$ to $r = 0.80$ in the new-born infants, of the order $r = 0.80$ in men and $r = 0.60$ in women. Analysis by means of partial correlation coefficients indicates that both stature and body weight have independent significance as bases for the prediction of the basal metabolism.

The change in basal metabolism with age during the period of adult life has been shown to be well represented by the linear equations,

For men ($N = 136$)

$$h = 1823.80 - 7.15, a, \; h_k = 28.703 - 0.112\, a, \; h_d = 1022.17 - 3.60\, a.$$

For women ($N = 103$)

$$h = 1420.47 - 2.29\, a, \; h_k = 28.308 - 0.124\, a, \; h_d = 942.25 - 2.96\, a.$$

where $h$ = total heat production in calories per 24 hours, $h_k$ = calories per kilogram of body weight, $h_d$ = calories per square meter of body surface as estimated by the Du Bois height-weight chart. Thus in men the daily heat production decreases about 7.15 while in women it decreases about 2.29 calories per year. Women are smaller than men and have a lower heat production. When the decrease in metabolism with age is expressed in calories per kilogram of body weight or in calories per square meter of body surface, the results for the two sexes are much more nearly identical.

The problem of the difference in the metabolism of men and women, dealt with in the past by a number of writers, has been reconsidered on the basis of the larger series of data now available. The average daily (24 hours) basal heat production of men is 1632 calories whereas that of women is 1349 calories. Thus women have an average daily heat production about 300 calories less than that of men. But women are smaller than men. If correction for body size be made by expressing heat production in calories per kilogram of body weight, it is 25.7 calories in the 136 men as compared with 24.5 calories, or 1.2 calories per kilogram less, in the 103 women. On the basis of heat production per square meter of body surface as estimated by the Du Bois height-weight chart the men show an average daily heat production of 925 calories as compared with 850 calories, or 75 calories less, in the 103 women. The most critical test of the difference of men and women in the level of metabolism is that furnished by a modification and extension of the selected group method of Benedict and Emmes. In the new method the control values for the several groups of women are not the empirical constants for men of as nearly as possible like stature and body weight but are determined from equations taking into account stature, weight and age in all the available data for men. Analysis shows that, however expressed, the metabolism of American women is lower than that of the men. Our results show that the differentiation of the sexes is not evident in infancy. They do not confirm the conclusion of Sondén and Tigerstedt that the difference between men and women tends to disappear with age. Instead we find the difference in the metabolism of men and women well-marked throughout the period of adult life.

The validity of the so-called body surface law, according to which metabolism is proportional to the superficial area of the individual, i.e.,

$$h = a\bar{h}_a$$

where $a$ = superficial area and $\bar{h}_a$ = mean heat production per unit of time per square meter of body surface in a standard series, has been critically tested. It has been shown that the supposed proofs of its validity hitherto adduced are erroneous. Heat production is not 'proportional to body surface but not to body weight' as has been asserted to be the case, but is highly and about equally correlated with both body weight and body surface. It has been shown that as a basis for predicting the heat production of a subject the above relationship is less satisfactory than multiple regression equations involving stature, weight and age. Thus the 'body surface law' is deprived of its unique significance as a basis for the prediction of the metabolism of an unknown subject. An analysis of the data of actual experimentation on subjects at changing levels of nutrition shows that the changes in metabolism are not proportional to those in body surface. Surface area may not be looked upon as a determining factor in basal metabolism.

The closest prediction of the daily heat production of a subject can be made by the use of the multiple regression equations,

For men, $h = 66.4730 + 13.7516\ w + 5.0033\ s - 6.7550\ a$

For women, $h = 655.0955 + 9.5634\ w + 1.8496\ s - 4.6756\ a$

where $h$ = total heat production per 24 hours, $w$ = weight in kilograms, $s$ = stature in centimeters, and $a$ = age in years. These equations have been tabulated for values of weight from 25.0 to 124.9 kgm., for stature from 151 to 200 cm., and for age from 21 to 70 years, so that the most probable basal metabolism of an unknown subject may be easily determined.

Such tables should render service in clinical and other fields of applied calorimetry. Their usefulness has been demonstrated in testing the typical or atypical nature of series of metabolism measurements, in investigating the differentiation of the sexes with respect to metabolic activity, of the metabolism of athletes as compared with non-athletic individuals, and of individuals suffering from disease.

The detailed measurements and statistical constants, with full discussions of pertinent literature, are about to appear in Publication No. 279 of the Carnegie Institution of Washington.

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## *SEX AND SEX INTERGRADES IN CLADOCERA*

By Arthur M. Banta

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Communicated by C. B. Davenport, October 18, 1918

There is probably no one factor in the biological world of greater interest and importance than sex.

Most plants are dioecious, producing the reproductive cells of both sexes within the same individual organism. Several lower animals are normally hermaphroditic but by far the larger part of the forms in the animal world are unisexual.

So general is unisexuality in animals and so little thought do we in general give to the comparatively few normally hermaphroditic forms that we are accustomed to think of maleness and femaleness as opposed and mutually exclusive states which cannot coexist in the same organism. We are accustomed to think of maleness as indicating the possession not only of a sperm-producing gland and accessory reproductive structures but also the possession of the peculiar secondary sex character—structural, physiological, and even psychological and behavior characteristics. The term female implies pronounced characteristics contrasting with those of a male on these several points. In other words maleness and femaleness are generally assumed to indicate definite and precise alternative and opposing states only one of which may obtain in the same individual organism.

With unisexual forms known deviations from the supposed mutually exclusive conditions of maleness and femaleness were until recently confined to a comparatively few cases of hermaphroditism and gynandromorphism. Such cases have been defined as sex mosaics—definite gonads or portions of the gonads or other parts of the body being described as distinctly of one or of the other sex.

However, there are at least three known cases of the common occurrence of unmistakable intermediate sex forms—sex intergrades. These are Riddle's hybrid pigeons,[1] Goldschmidt's hybrid gypsy moths[2] and the writer's Cladocera material. These are not sex mosaics but sex intergrades.[3] That is to say, they are not fully male in certain definite portions of the body and fully female in other portions but frequently as a whole and (in the writer's material) almost always in certain parts they clearly possess both femaleness and maleness—they are as a whole and by parts distinctly intermediate as regards sex.

In the Cladocera material there sometimes occurs an individual which might very readily be considered a sex mosaic, inasmuch as its complement of secondary sex characters consists of some apparently fully male and some fully female characters. But the majority of these secondary sex characters are obviously intermediate between the fully female and the fully male character, and there are comparatively few of the sex intergrades which do not have one or more such intermediate sex characters. These intergrading sex characters clearly indicate that we have to do not with sex-mosaics but with sex intergrades.

In Cladocera the population ordinarily consists entirely of females and reproduction is by means of parthenogenetically developing eggs. Occasionally males appear and some females produce the less usual type of egg which must be fertilized in order to develop. This occasional sexual reproduction has given rise to the notion that there is an innate sexual cycle in these forms.

In all the many lines of the writer's stock however, previous to the appearance of the sex intergrade strains, males were seen only two or three times and in as many lines.[4] Though no sexual reproduction has occurred in any case the various lines continue to reproduce with their original vigor. Hence as applied to this material the supposed innate sexual cycle seems quite an unwarranted assumption.

In the writer's stock previous to the appearance of the recognized sex intergrade strains there were a few individuals of one strain of Daphnia longispina which may have been sex intergrades. These individuals failed to reproduce but it is probable that they were really sex intergrades and that there would have been intergrades among their offspring if they had reproduced at all.

Undoubted sex intergrades were first found and propagated in the writer's stock in one of the strains of Simocephalus vetulus. None have been seen in any strain of this species except in direct descendants of the initially discovered sex intergrade mother. Great numbers of Simocephalus vetulus have been examined microscopically especially in the last sixteen months and while normal males have been found on occasion, no sex intergrades have been discovered. This is true in spite of the fact that the sex characters are the characters especially scrutinized and that in the aggregate many thousands of Simocephalus vetulus have been examined. Hence in Simocephalus vetulus the occurrence of clearly marked sex intergrades is by no means to be considered a frequent phenomenon.

In Daphnia longispina, however, sex intergrades have been discovered in all three of the strains of line 768, and we now have in the laboratory well established strains of sex intergrades from two of these three strains. Sex intergrades have been found also in two of the three other laboratory lines of this species. Coupled with the fact that the observation of probable sex intergrades at an earlier time was also in this species, the occurrence of sex intergrades in all except one of the six strains of Daphnia longispina in the laboratory, seems to indicate that for this species the production of sex intergrade strains is not such an unusual phenomenon. However, one significant fact should not be overlooked, namely, that except for close scrutiny of the cultures including microscopic examination of great numbers of individuals the occurrence of intergrades would not have been suspected.

Though in recent months males have been found in all except one strain of all the stock of the seven species of Cladocera reared in the laboratory no intergrades have been discovered in either Daphnia pulex, Simocephalus serrulatus, or in three species of Moina, or in Simocephalus vetulus except in descendants of the original sex intergrade mothers. The sexual characters of thousands of individuals of each of these species have been microscopically examined.

Within the stock in the laboratory it then seems fairly clear that the occurrence of sex intergrades is quite unusual, though intergrades have been found in most of the strains of Daphnia longispina.

The sex intergrade strain of Simocephalus vetulus originated in October, 1915, in line 740 in its 131st laboratory generation during a period of conditions of poor nutrition in the stock due to unsatisfactory food.

The secondary sex characters and other matters relating to this intergrade strain have been discussed in some detail in another paper[5] (Banta, 1916) and need not be considered further here.

The sex array is extensive. There occurs almost every combination of male and female primary and secondary sex characters. This coupled with the fact that most of the secondary sex characters occur very frequently in an intermediate condition serves to make the sex array in Simocephalus very extensive indeed. In fact a precise classification is quite impossible and only an extremely relative classification is at all feasible.

Since the origin of sex intergrades in line 740, the sex intergrade strains have descended for nearly three years (about 57 generations). The character of the stock seems unchanged—the sex array and proportions of the various apparently pure sex forms and intergrades of different sorts continues as it was when the intergrade stock first appeared.

The sex intergrades in Daphnia longispina were first observed in May, 1917. They appeared in one of the three strains of line 768. This strain was not passing through a period of unfavorable conditions at that time nor was there anything unusual observed in its behavior except the occurrence of sex intergrades. It is indeed not certain that intergrades may not have occurred sparingly for a few generations before they were discovered. The first intergrade mothers noted in this strain produced many sex intergrades among their offspring. Several intergrade strains were at once isolated from this stock and placed under observation.

The secondary sex characters in Daphnia longispina are as follows: (1) *Body size*—the females are larger than the males; (2) *Character of the head outline*—the ventral margin of the head in the female slopes almost uniformly to the tip of a pointed rostrum or beak while in the male the beak is absent; (3) and (4) *Characters of the first (rudimentary) antennae*—the paired female first antennae are represented only by slight eminences from which arise a few sensory stylets, while in the male the first antennae are movable structures three-fifths as long as the head. At its distal end each male antenna is armed with a stout bristle in addition to the sensory stylets possessed by the female; (5) and (6) *Character of the breast margins*—the female ventral carapace margins are almost uniformly rounded from the anterior portion backward. In the male these margins are angulated almost to 90 degrees in the region nearest the head. Further in the region of this angle these margins are fringed with hairs in the male while none occur in the female; (7) and (8) *Character of the first legs*—in the female the terminal joint of each of the first pair of legs is not armed with a hook, while this segment in the male bears a stout hook.

That these characters are distinctive of the two sexes is shown by the fact that they occur practically without deviation in wild stock, and in females[6] and males of the stock not producing intergrades, as well as in the extremes of the series in the sex intergrade strains.

The array of sex intergrades in Daphnia longispina is less extensive than in the sex intergrade strains of Simocephalus vetulus in that male sex intergrades (i.e., intergrades with testes) are almost or quite lacking and further that males are extremely scarce in the sex intergrade strains of this species. That is to say that toward the male end of the series where should occur numbers of male intergrades and ostensibly normal males—if the sex array were complete and uniform—the numbers of male intergrades and apparently fully male individuals are few and the series is not at all diverse. On the other hand with the Simocephalus vetulus sex intergrade stock normal males are abundant and male intergrades are common in occurrence. In other words the sex intergrade strains of Daphnia longispina are more female and correspondingly less male than those of Simocephalus vetulus.

In another manner the sex intergrades of Daphnia longispina differ from those of Simocephalus vetulus. Each of the several secondary sex characters in Daphnia longispina is subject to intermediate development and occurs in all intergrading stages from the fully female to the fully male secondary character; whereas some of the secondary sex characters in Simocephalus vetulus are not such as to readily show so wide a range of intermediate conditions.

One rarely finds a Daphnia longispina intergrade which has its full complement of secondary sex characters—each character apparently fully male or fully female. The vast majority of the Daphnia longispina intergrades have at least three (of a total of eight) secondary sex characters distinctly intermediate between the male and female in point of sex significance, while frequently all of the secondary sex characters partake of the intermediate condition.

With the sex intergrade strains of both Simocephalus vetulus and Daphnia longispina the character of offspring is somewhat correlated with the secondary sex characters of the mother. The more maleness the female intergrade possesses the more highly male in general will be the character of the offspring. In most cases the relative amount of maleness and femaleness possessed by the different individuals of a brood of young is extremely variable; but ordinarily if the mother is highly intergrade in several characters the young, while quite variable will also on the average be relatively highly male in their characteristics. On the other hand a female from a sex intergrade strain who herself shows little or no evidence of maleness will ordinarily produce young with few or slight male characters, though the young are on the average usually more male than the slightly intergrade mother herself.

An extremely intergrade mother, whose secondary sex characters are all largely or fully male, is in most cases sterile or nearly so. This is notably true of the Simocephalus vetulus sex intergrades in which a female intergrade with a full complement of male secondary sex characters has not been known to reproduce. On the other hand a female intergrade with few male or only slightly male secondary sex characters is usually highly productive of young.

Sex intergrade production would seem to be the result of a disturbed balance, a condition which—obviously in many individuals and probably in all individuals, *at least* of the sex intergrade strains—is a struggle of two nearly equal factors or sets of factors, the one making for maleness, the other for femaleness. The result of this struggle of factors is individuals ostensibly male in part and female in part and obviously intermediate in part—but as a whole distinctly intermediate in sex characters.

The point of perhaps greatest importance lies in the findings of sex intergrades in a number of lines of a second species of Cladocera—in fact in five out of six lines of that species which have been reared in the laboratory. In addition De la Vaulx records[7] what are almost certainly such intergrade individuals in Daphnia atkinsoni. These extensions of the known occurrences of sex intergrades among Cladocera indicate that a condition of sex intergradedness in Cladocera is less unusual than the writer formerly supposed—that maleness and femaleness are even less exclusive phenomena so far as indicated by morphological characters in Cladocera than was believed. In this material the disturbances of the balance of the factors or sets of factors making for sex is less unusual than would seem probable if maleness and femaleness were really generally mutually exclusive.

From such clear cases of sex intermediates one wonders if maleness and femaleness are really mutually exclusive in those Cladocera individuals which morphologically show no unlike sex characters. It seems extremely probable that they are not, particularly in view of the fact that in sex intergrade strains apparently normal females frequently produce young of widely divergent types of sex intergrades. Even in the 'normal' strains one is certainly justified in thinking that maleness and femaleness are not complete and mutually exclusive states but that in these apparently normal sex forms, too, sex is also relative—differing from the so-called sex intergrades not in kind but merely in degree, not qualitatively but quantitatively.

With the relativity of sex so emphatically shown in hybrid pigeons, in hybrid moths and in different species of Cladocera one wonders if the relativity of sex ends with pigeons, gypsy moths and water fleas. There seems every reason to think it does not. We are coming to the time when it would seem imperative to revise our notions of the fixity of sex. The clear cases of sex intergrades or sex intermediates just referred to seem no more nor no less illustrations of the relativity of sex than one sees in the 'crowing hen' and the 'sitting cock' or in the masculine woman and in the man who in almost intangible physical characteristics, in speech, in dress, in tastes and habits of behavior, and in methods of thought reveals himself as lacking in something which makes for the fully equipped male and as possessing qualities ordinarily recognized as characteristics of the female.

Further it does not seem necessary to suppose that relativity of sex is restricted to cases in which its very conspicuousness forces itself upon our unwilling attention in opposition to our fixed conceptions of maleness and femaleness as complete, opposed, and mutually exclusive phenomena. Indeed the more reasonable supposition is that sex is always relative, that while most sexual individuals of whatever species are prevailingly male or prevailingly female every individual may have something of the other sex intermingled with its prevailing sexual characters.

[1] Riddle, Oscar, *Carnegie Inst. Washington Year Book*, **12**, 1913, (322); also *Amer. Nat.* **50**, 1916, (385–410).

[2] Goldschmidt, R., *Zs. ind. Abs.-Vererbungslehre*, **7**, 1912, (1–62); also these PROCEEDINGS **2**, 1916, (53–58).

[3] Called sex intermediates by Riddle; intersexual forms by Goldschmidt.

[4] There is one reference in the literature to androgynous Cladocera (in Daphnia atkinsoni). Judging from De la Vaulx's description these individuals, which he called gynandromorphs, were almost certainly sex intergrades.

[5] Banta, A. M., these PROCEEDINGS, **2**, 1916, (578–583).

[6] The writer used the terms female and male with the mental reservation that inasmuch as femaleness and maleness are now definitely known to be relative in some cases they may be relative in *all individuals* and that the terms female and male are themselves to be considered relative not only in Cladocera, pigeons and moths but perhaps also in all animal and plant forms in which sex is known to occur.

[7] Vaulx, R. de la., *Bull. Soc. Zool. France*, **40**, 1916, (102–104, 194–197,

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# *ON THE METHOD OF PROGRESSION IN POLYCLADS*[1]

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Communicated by E. L. Mark, October 29, 1918

Turbellarians are able to make progress through the water by means of muscular movements of the body as a whole, or of its lateral extensions; their creeping progression on a solid substratum, however, has usually been attributed to the action of cilia. According to Stringer (1917),[2] the locomotion of planarians is effected by muscular contractions, which may be organized after the manner of the locomotor wave upon the foot of a monotaxic gastropod (cf. Parker, 1911),[3] the ciliary activity taking no necessary part in the locomotion. Some observations, which may here be briefly noted, tend to confirm the nature of this finding, and add to the variety of known muscular creeping movements in turbellarians.

One species of Leptoplana, occurring at Bermuda in moderate numbers, differs from the more frequently encountered L. lactoalba Verr., approaching more nearly the form L. lactoalba var. tincta Verr. It is found on the under surfaces of stones, near high water level, and is commonly about 3.5 cms. long. If, out of water, a stone bearing one of these leptoplanas is turned over, exposing the animal to light, it creeps about upon the moist surface. The worm also creeps rapidly under water, and in addition is a vigorous swimmer. It has usually been believed that in Leptoplana "creeping is a uniform gliding movement, caused by the cilia of the ventral surface, aided perhaps [how?] by the longitudinal muscle layer of this surface" (Gamble, 1901, p. 10).[4] While this may very well seem to be the case in L. tremellaris (Gamble, loc. cit.), and in several other species which I have observed, it is distinctly not true of the form which I may refer to as L. 'tincta.' The ventral surface of this platode is richly ciliated, and in creeping it deposits an appreciable slime-track, such as that in which the cilia of triclads have been supposed to cause creeping owing to their beatings. But the muscular operations involved in the creeping are of so obvious a kind as to be quite unmistakable. These movements are the same whether the worm is in water or out of it, whether it is creeping upon the surface film or on submerged objects.

The essential feature of the progression is illustrated in Figures *A*, *B*, *C*. Creeping is rapid, as quick as 5 mm. per second. One side of the body at the anterior end is lifted from the substratum, thrust 4 to 5 mm. forward, and firmly attached to the substratum; then the opposite side is thrust forward in a similar way. On each side of the body a wave is thus initiated, which may travel the whole length of the animal or may go only one-half to one-third the distance toward the tail. The edge of the body is thrown into wavy wrinkles; these progress toward the posterior end, which is reached in 3 to 4 seconds. The succession of these waves is rapid, so that 4 to 5, or more, may be present at one time on each side of the body. This rapidity makes it difficult, except at the anterior end itself, to be sure in all cases of the truly alternate relation of the waves on the two sides; but in many instances they undoubtedly remain alternate, especially in slow creeping. The shuffling movement of the worm under these circumstances recalls vividly that of a retrograde alternate ditaxic gastropod foot.[3]

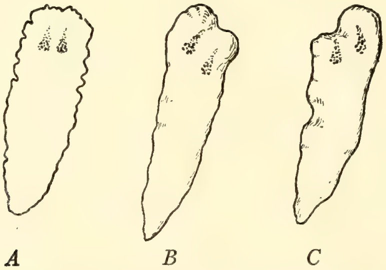

FIG. 1 ILLUSTRATING THE METHOD OF CREEPING IN A SPECIES OF LEPTOPLANA. DIAGRAMMATIC.

*A*, at rest; *B*, *C*, the initation of alternate waves of body contraction which are responsible for creeping. (Natural size.)

When Leptoplana 'tincta' is induced to creep with special rapidity, the body-waves become more pronounced, and although they remain distinctly alternate and 'ditaxic' in their origin at the anterior end of the animal, they may fuse to form a single retrograde, 'monotaxic' wave after about a third of the worm's length has been traversed. The locomotor wave thus becomes more allied to that developed in swimming, for although some other leptoplanas swim through the water by means of winglike flappings of the lateral extensions of the body,—a process analagous to the parapodial swimming of Aplysia—in L. 'tincta' the swimming movement involves a sinuous longitudinal 'gallop' of the body as a whole.

During progression on a surface, the periphery of the body is the part vitally concerned, for the animal may be transversely arched in such a way that, save at the very anterior end, the periphery is the only part actually in contact with the substratum. The attachment of the peripheral edge of the worm is produced by local suction. Although slime is secreted there, the manner in which the attachment is made to a small surface, e.g., a pencil point, and the way this attachment may be instantaneously released when the worm begins to creep, force one to believe that the edge of the body is not being held in place by sticky slime. This use of the edge of the 'foot' is similar to that found in Chromodoris, and perhaps in other large midibranchs (Crozier and Arey, 1918).[5]

At rest, the outline of L. 'tincta' is usually thrown into 'stationary waves.' These waves may appear truly opposite, not alternate, especially at the anterior end of the worm. Occasionally, at the beginning of creeping, a 'monotaxic' movement is executed, but this operation is characteristically 'ditaxic,' the lateral halves of the body contracting in alternate sequence.

With one of the several species of Pseudoceros which are found inhabiting colonies of the tunicate Ecteinascidia turbinata Herd., I have been able to make observations which show that muscular creeping operations are probably general among polyclads. In this form also the locomotor waves are essentially alternate, 'ditaxic,' and retrograde, the axial region of the body being usually arched away from the substratum, thus separating the body into halves. The wave is a region lifted from the substratum. Two waves may be present on each side, simultaneously. Here, as in the Leptoplana, *when the muscular waves are absent, no creeping progression can be detected.* In the Pseudoceros the waves are especially well seen when the animal is creeping on the surface film. A rippling movement of the outline of the body usually accompanies the 'body-waves,' and when viewed from above this is the only contractile operation to be seen. The peripheral rippling may, however, be very slight or even absent altogether.

Neither of these flat-worms has been observed to creep in any but an anterior direction.

These observations tend to show that in turbellarians generally, muscular operations analagous to those executed by the foot of chitons and of gastropods are essentially concerned in creeping locomotion.

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[1] Contributions from the Bermuda Biological Station for Research. No. 89.

[2] Stringer, C. E., 1917, These PROCEEDINGS, **3**, 691–692.

[3] Parker, G. H., 1911, *J. Morph., Wistar Inst., Phila.*, **22**, (155–170).

[4] Gamble, F. W., 1901, *The Cambridge Nat. Hist.*, **2**, p. 1–96, 47 fig., London

[5] Crozier, W. J., and Arey, L. B. *The sensory behavior of Chromodoris zebra.* (In press).

# THE PHYLOGENY OF THE ACORN BARNACLES

By Rudolf Ruedemann

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Communicated by J. M. Clarke, October 29, 1918

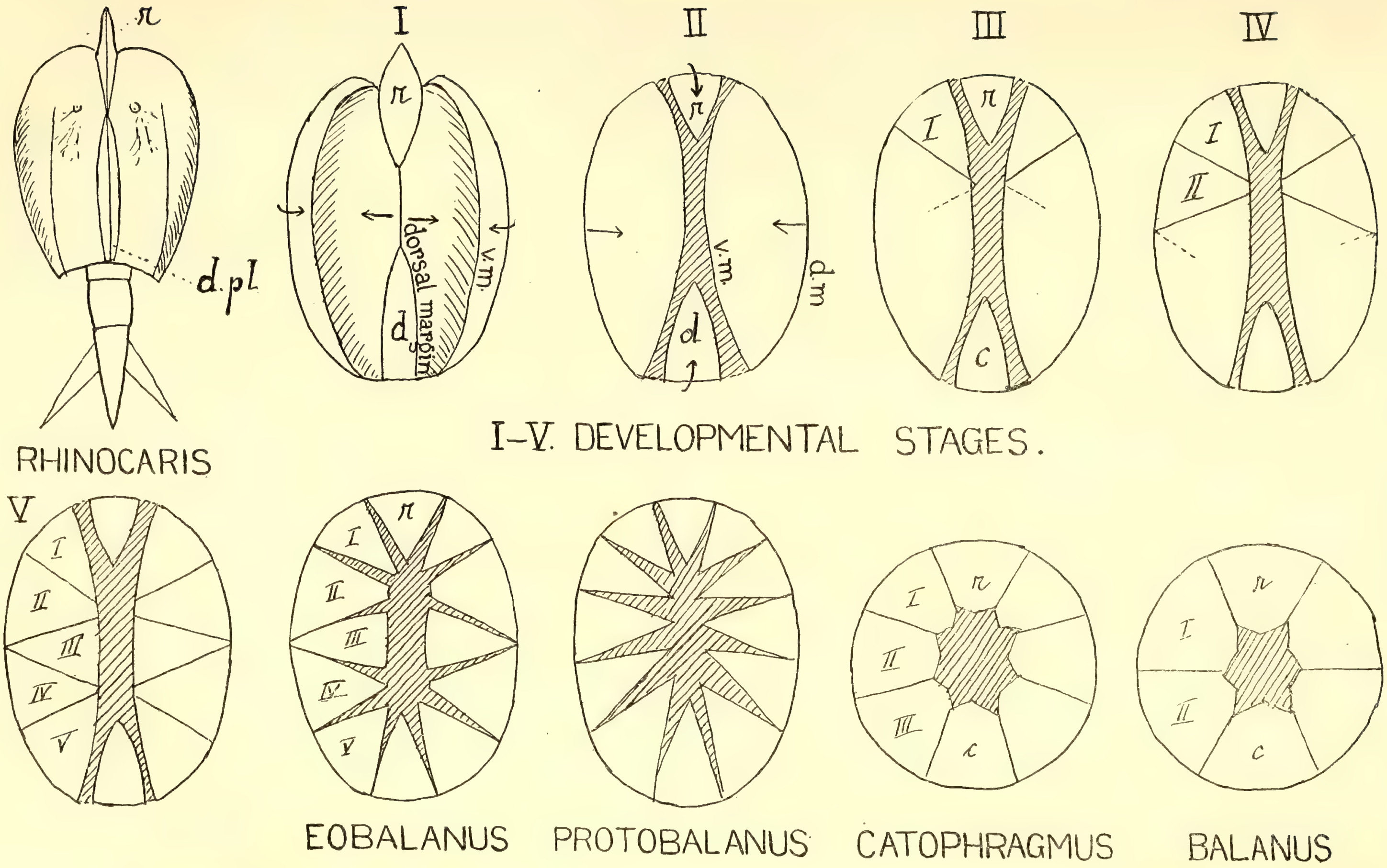
The Acorn Barnacles of Balanidae represent the most advanced stage of regression attained in the Cirripedes through adaptation to a sessile life. Darwin regarded them as derived from the Lepadidae or Goose Barnacles by the loss of the peduncle, and this view has been generally accepted. The whole order of Cirripedia is interpreted by some students as derived from the Copepoda, but others find its ancestors in the ostracodes, because of the bivalved 'Cypris-stage' through which the larva passes after the Nauplius-stage and directly before attachment. Balfour, however, in his treatise on *Comparative Embryology*, 1880, p. 424, emphasizes the presence of paired, compound eyes as well as the large bivalve shell, and has urged their "independent derivation from some early bivalve Phyllopod form."

It has further been recognized that a reduction in the number of the compartments (wall plates) or lateralia has taken place, probably by coalescence. The typical Balanus has four lateralia, two on each side; besides these the rostrum and carina, or six compartments in all. But the most generalized living genus Catophragmus has eight compartments, or three lateralia on each side. Paleontologists know in the Devonian fauna Protobalanus hamiltonensis Whitfield, an acorn barnacle with 12 compartments or 5 lateralia on each side

Investigations of the Utica and Lorraine faunas of the Upper Ordovician have recently brought to light a new form of acorn barnacle, which like Protobalanus, has five lateralia on each side, but differs in the form of these lateralia in such a way as to shed a most important light on the problem of the origin of the barnacles. In Protobalanus the five lateralia, which are all acutely triangular plates, point uniformly upward or inward towards the mouth. In the new form, which will be described as a species of the new genus Eobalanus, these plates are arranged as shown in the diagram, the four outer lateralia as in Protobalanus, while the middle one is inverted, forming a sort of keystone in the series. The second and fourth are furthermore somewhat truncated at the apex.

The effect of this peculiar shape of the lateralia is that, if they could be matched together like the parts of a picture puzzle, they would give a perfect, snugly-fitting carapace, as shown in diagram V. This carapace is bivalved as in phyllopods, with the rostrum and carina of the barnacle corresponding to the rostral plate of the crustacean in front and the 'dorsal plate' behind, exactly as in the Devonian genera Mesothyris and Rhinocaris.

We have then to picture the derivation of an Eobalanus as from a Rhinocaris-like phyllopod as illustrated in the set of diagrams. The first diagram


n
d.pl
RHINOCARIS
I
n
dorsal margin
v.m.
d
II
n
v.m.
d.m
d
III
n
I
c
IV
I
II
I–V. DEVELOPMENTAL STAGES.
V
I
II
III
IV
V
n
I
II
III
IV
V
EOBALANUS
PROTOBALANUS
n
I
II
III
c
CATOPHRAGMUS
n
I
II
c
BALANUS

shows the carapace of the crustacean just before attachment with the head portion and the dorsal side downward—a rather natural position for phyllopods, which, like Apus, are wont to swim on their backs, while foraging along the bottom.

The head and back being thus protected by attachment, but the ventral side open to attack, the next step will be the separation of the carapace valves along the hinge line and their movement upward towards the ventral side; and likewise the rostral and dorsal plates will have to move upward to fit in again between the valves (stage II of diagrams). Following this was the breaking up of the valves into the lateralia, owing to stresses exerted at one or other end, possibly the anterior one where the originally chitinous and somewhat flexible valve was attached. Here also, our material affords a clue to the mode of procedure. A very early growth stage of Eobalanus shows four radially arranged, subequal, oval plates, the two lateral ones of which show a suture along which a smaller part is being split off. It is thus to be inferred that the compartments were formed by successive splitting off of plates from the original valve, each fissure producing a new pair of lateralia. In this way the peculiar interlocking arrangement of the compartments in Eobalanus would finally have come about and each valve of the carapace have been divided up without leaving a useless remainder.

The scuta and terga which form the valvular carapace or operculum of the upper aperture of the later Balanidae and Lepadidae and which are of great taxonomic importance, have not been found in Eobalanus and Protobalanus, and in our view did not exist then, but are a later development to close in more completely the ventral side. They are not fundamental structures.

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# *POSSIBLE DERIVATION OF THE LEPADID BARNACLES FROM THE PHYLLOPODS*

By John M. Clarke

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Communicated, October 29, 1918.

Mr. Ruedemann's discovery of an elemental balanid and his constructive deduction therefrom of the origin of the acorn barnacles, sets this form of symbiotic degeneration back to an historic stage, where its phylogeny is indicated in its own structure. The degeneration is already complete and hence implies a long time for the acquirement of such adaptation, but it is also so simple as to indicate its procedure. If Eobalanus intimates the rise of the Balanidae through the decline and dependence of the Phyllopods, it is opportune to regard the evidence bearing on the phylogeny of the other great division of the barnacles, the Lepadidae or Goose Barnacles.

There is a long lapse in the time record of this line of development for we have no present knowledge of the lepad barnacles, in the marine interval between the Jura and the Devonian. Certain rare fossils known as Turrilepas and Strobilepis have been found in the rocks of the Devonian, Silurian and Ordovician; Strobilepis is a singular genus constructed of four vertical ranges of plates, two rows broadly triangular and the other two narrow and spiniform, and the fact that this Devonian genus is known in but a single specimen prevents us from going far in utilizing it in this connection, but its structure, a scaly elongated body ending in a single subcircular plate from which the vertical ranges diverge, indicates that this plate is terminal at the free end of the animal; in other words, a caudal plate. In my previous discussions of this genus I have placed Strobilepis and Turrilepas together under the family Turrilepadidae, Turrilepas being the older in time and date of description and having similar structure except that the vertical ranges are from four to six, the scales of each range being more or less interlocked with those adjoining. This fact and the absence, so far as known, of a terminal or caudal plate, has led some students to regard Turrilepas as the scaly stalk of a lepad rather than the capitulum. However this may prove, I think we may be obliged to conceive that there is a less close relation between these two genera than their gross structures indicates.

In the Silurian and Ordovician we have the genus Lepidocoleus now standing as the representative of the family Lepidocoleidae and in this the type of structure is much simpler than in the genera mentioned. Here is an elongate body consisting of but two vertical ranges of plates, which are as distinctly 'lateral' in a morphological sense as are the lateralia of the balanids. These two rows open back and front making two continuous lines of suture or dehiscence from the base of attachment to the summit, where there is a terminal axial plate. The lateral plates are subequal half-rings or bands and the Lepidocoleus was attached to the surface on which it grew by its end and obviously by its head. My recorded observations on these structures in the *Palaeontology of New York*, VII, the *American Geologist*, *American Naturalist* and Eastman-Zittel's *Text Book of Palaeontology* are sufficient demonstrations of these relations.

Mr. Ruedemann's hypothesis of derivation predicates attachment of the ancestral phyllopod by its head and back; primarily by its head, secondarily and subsequently by its back. If we limit this fixation to attachment by the head alone leaving the rest of the body entirely free and hence subject to much more frequent and pronounced lateral stresses resulting from movements, by waters or otherwise, from side to side, we call into play only such stresses as reasonably seem to have been effective in the development of the Phyllocardia into Eobalanus.

The arguments and the causes are of the same category as those which my associate has employed. It is, therefore, a matter of special and confirmatory significance that these Lepidocoleus forms appear in the Ordovician; that is to say that so far as our present knowledge extends the metamorphoses of the Phyllopods into the two great branches of the barnacles were essentially contemporaneous.

## *REFRACTIVE INDEX AND SOLUBILITIES OF THE NITRATES OF LEAD ISOTOPES*

By Theodore W. Richards and Walter C. Schumb[1]

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Communicated October 31, 1918

The refractive index of the nitrate of pure common lead was compared very carefully with that of the nitrate of uraniolead prepared from the Australian material already mentioned in previous reports. The two samples of nitrates were separately recrystallized with care, and finely developed crystals were measured in the Abbé-Zeiss total-reflecting crystal refractometer. A solution of sulphur in methylene iodide having a refractive index of 1.79 was used as the contact liquid, and very careful measurements were made of many crystals in varying positions. The refractometer was standardized by means of a polished glass prism possessing an exactly known refractive index which could be verified by the method of minimum deviation. The refractive index of common lead nitrate at 20° was found to be 1.7815, and that of the uraniolead nitrate was found to be 1.7814—each value being a mean of many concordant determinations. Thus the difference in atomic weight shown by the two specimens of lead (the atomic weights are respectively 207.20 and 206.41) has no appreciable effect on the refractive index of the salt—a highly interesting and newly discovered fact.

The two specimens of lead nitrate which had been purified (crystals of which had been used in the preceding investigation) were further investigated with great care as to solubility, using a method recently described,[2] with small but convenient improvements. Greater difficulty was found in obtaining an exactly saturated solution than was the case with sodium sulphate, at least twenty-four hours at perfectly constant temperature being needed in order to obtain constant results. The weighed portions of solution saturated at 25.02° were evaporated with excess of sulphuric acid and the sulphate was gently heated at 350° until constant in weight. Four such determinations with ordinary lead nitrate gave values from 37.33 to 37.36 (average 37.342) grams of nitrate in 100 grams of solution, and nine determinations with uraniolead nitrate gave values from 37.26 to 37.30 (average 37.280) grams of nitrate in 100 grams of solution. These results are precisely proportional (within the limit of error of experiment) to the different molecular weights of the two samples of nitrate. Expressed in other terms, the molal solubilities of the two samples per thousand grams of water are respectively 1.7993 for common lead nitrate and 1.7991 for uraniolead nitrate. The outcome of this work, then, by finding no appreciable difference in the molal solubility of the different samples of lead nitrate containing isotopes of widely different atomic weights, confirms earlier work upon other properties of these interesting substances. Evidently weight (or mass) is the prime distinguishing feature of the two kinds of lead here studied, as it was in other cases. This work and that described in the preceding section thus afford further support for the hypothesis due to Russell, Fleck, Soddy and Fajans.

[1] This paper and the two following are taken from an abbreviated version of a report to the Carnegie Institution of Washington (which generously subsidized the investigations) presented in August, 1918.

[2] Richards and Yngve, *J. Amer. Chem. Soc., Easton, Pa.*, **40**, 1918, (164).

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# *THE PURIFICATION BY SUBLIMATION AND THE ANALYSIS OF GALLIUM CHLORIDE*

By Theodore W. Richards, W. M. Craig, and J. Sameshima

Wolcott Gibbs Memorial Laboratory, Harvard University

Communicated, October 31, 1918

A new method for the purification of gallium salts was worked out in some detail. This rests upon the convenient fact that gallium trichloride sublimes and distils at a low temperature, whereas the other chlorides likely to be associated with it are much less volatile. The method rested, therefore, upon fractional distillation and sublimation, at first in a stream of chlorine and afterwards in a vacuum, of impure gallium chloride.[1] The apparatus was a complex affair, in which gallium could be first burned in pure dry chlorine and then subjected to distillation either first in pure chlorine, then in nitrogen, and finally in a vacuum, the whole apparatus being fused together without rubber connections and scrupulously dried. In order to avoid the use even of ground joints (with their attendant alternative danger of leakage or contamination from lubricant) the gas connections were opened by means of sealed magnetic hammers acting on enclosed capillaries, and closed by fusion of the glass connections. The gallium was provided through the great kindness of Mr. F. G. McCutcheon of the Bartlesville Zinc Company, Blackwell, Oklahoma, to whom grateful thanks are due. Three successive distillations of the trichloride of gallium were made in chlorine at 220° to 230°C., three more at about 175° (the melting point of gallium dichloride), three in nitrogen at 90° to 110°, and five sublimations in vacuo at 65° to 80°—fourteen in all. Distillation in nitrogen or in vacuo is needful to eliminate dissolved chlorine. The resulting product showed no trace of any other substance in its spark spectrum when examined with great care in a Hilger wave length spectrometer. Although this study did not reach a final stage, and much more remains to be done in order that perfect certainty may be attained, nevertheless the outcome must be regarded as promising in pointing toward an adequate and convenient method of separating gallium from other metals.

Three fractional samples of the chloride discussed in the previous section were analyzed, using the usual methods employed in Harvard University in order to make a preliminary determination of the atomic weight. The samples were small and for various reasons the results cannot be considered as anything more than preliminary, but the outcome is, nevertheless, worth recounting. In the last and best determination 0.43947 gram of gallium chloride (weighed in vacuo in a sealed glass tube) yielded 1.07087 grams of silver chloride having required 0.80587 gram of silver for complete precipitation. The atomic weight of gallium computed from these two sets of data are respectively 70.09 and 70.11—concordant results indicating a value somewhat higher than that usually accepted for gallium, but near enough to show that the chloride was at least not far from being pure, and that the whole proceeding is capable, when employed with larger quantities of material and with the experience already gained, of affording an accurate evaluation of this atomic weight. Of course such meagre data as those thus far secured furnish no worthy evidence concerning it; they are as merely preliminary as the data of Lecoq de Boisbandrau. At the conclusion of the war it is hoped that the joint investigation (which was stopped by the departure of W. M. Craig into War Service) may be continued and completed.

[1] The principle of this method has since been published by Dennis and Bridgman, although the details of their treatment differed from ours. Their work was entirely independent of ours (which was brought to a close in March, 1918) and was entirely unknown to us. *J. Amer. Chem. Soc., Easton, Pa.*, **40**, 1918, (1540).

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# *THE PURIFICATION OF GALLIUM BY ELECTROLYSIS, AND THE COMPRESSIBILITY AND DENSITY OF GALLIUM*

By Theodore W. Richards and Sylvester Boyer

Wolcott Gibbs Memorial Laboratory, Harvard University

Communicated, October 31, 1918

The method of separating gallium from indium by means of the different solubilities of the hydroxides in caustic alkali, recommended by various authorities, was tested without success. The separation was found to be so incomplete that several per cent of indium remained in the gallium, at least under the conditions used in our work, and it seemed clear that this difference in solubility is not enough to effect a complete separation. Much more promising results were obtained by the electrolytic method. Gallium occupies a place in the electrolytic series between indium and zinc. It is far less easy to deposit than indium, but, on the other hand, much more easy to deposit than zinc. By carefully regulating the hydrogen-ion concentration and current density it was possible to deposit practically all of the indium with only a little gallium; and thereafter most of the gallium could be separated in a slightly acid solution without the appearance of an important amount of zinc. Gallium obtained in this way from material which had previously been purified by the hydroxide method, melted at a temperature as high as 30.8°, a higher melting point (indicating a purer substance) than is recorded anywhere in the literature. The metal obtained by the hydroxide process, without electrolysis, melted at a temperature as low as 26.9°. Time has as yet been lacking for making complete spectroscopic and other tests of this material in order to confirm its complete purity, but enough has been done to show that this method is a very satisfactory and convenient one. Because of its obvious advantages it must have been employed before by others, but no mention has as yet been found of it.[1]

Having thus prepared gallium of substantial purity, we proceeded to determine its compressibility, both in the solid and in the liquid condition. This was an interesting problem since comparatively few substances have been measured in both states. However, gallium has the remarkable and rare property of occupying more volume in the solid than in the liquid condition. The determination of the compressibility was desirable in order to add to the long list of elements already determined at Harvard. The apparatus and principle of the method was essentially similar to that employed in other cases, but a new difficulty was encountered. As in the case of other metals, the gallium could not, of course, be allowed to come in contact with the mercury in the piezometer, but on the other hand, it could not be conveniently solidified in any tube under such an inert liquid as toluene without bursting the tube. After many more or less satisfactory devices had been tried, the best results were obtained by solidifying gallium in the first place, and then placing a cylinder of the solidified metal in a slightly larger short test tube, just fitting it, and capping this with another similar slightly larger test tube under the inert liquid. Twenty-three grams of pure gallium were used in this work, and the compressibility was found to be $2.09 \times 10^{-6}$, placing gallium precisely on the curve joining the other compressibilities in the graph representing the periodic relation of this property to atomic weight. The compressibility of gallium containing several per cent of indium as obtained by the hydroxide method was found to be somewhat less ($1.97 \times 10^{-6}$). Liquid gallium was determined not only in this apparatus but also in one similar to that used for caesium (Publication of the Carnegie Institution of Washington, No. 76, p. 20). The liquid was thus found to have a compressibility of $3.97 \times 10^{-6}$, a value almost exactly identical with that for mercury, and nearly twice as great as that of solid gallium, although its volume is less. The determination was made at 30°. This confirms the universal experience that solids have compressibilities distinctly less than the same substances as liquids, entirely irrespective of the volumes which they occupy. The most marked case of this kind thus far observed is that of ice.[2]

The densities also of solid and liquid gallium were determined,—data which have especial interest because the expansion of gallium on freezing has been attributed by some investigators to impurity. In the first place careful determinations were made by means of a pycnometer for solids[3] of the density of the impure material containing some indium in the solid and liquid condition, the values found being respectively 5.975 and 6.166. Subsequently when the purest material had been obtained, the determinations were repeated with equal care, giving values 5.885 and 6.081 respectively. Evidently indium had produced no essential effect upon the expansion on freezing and even if the more carefully prepared gallium was not absolutely pure, it is evident from the outcome, by extrapolation, that the purest gallium must still possess this unusual property.

[1] This method also has since been published by Dennis and Bridgman in their interesting article on Gallium.

[2] Richards and Speyers, *J. Amer. Chem. Soc., Easton, Pa.*, **36**, 1914, (491).

[3] Richards and Wadsworth, *Ibid.*, **38**, 1916, (222).

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# *THE GROWTH-RATE OF SAMOAN CORAL REEFS*

By Alfred G. Mayor

Department of Marine Biology, Carnegie Institution of Washington

Communicated October 30, 1918

In April, 1917, reef corals from Pago Pago Harbor, American Samoa, were measured, photographed, weighed, marked by numbered brass tags and replaced in favorable situations upon the reef-flats either by tying them with wire to iron stakes or imbedding their bases in concrete.

In July, 1918, these corals were again studied in order to ascertain their growth-rate, and as the specimens selected were of average size it was hoped that an approximate determination of the average growth-rate for each species might be determined. It was our object to calculate the weight of stony matter which coral heads of average size add to the reef per annum. In order to do this, the weight of the living coral was ascertained both in 1917 and in 1918. Then, in 1918, the coral was killed and its animal substance dissolved in KOH, after which the fresh water was largely abstracted by washing the skeleton in 90% ethyl alcohol; and finally the coral was dried in the sun, and then weighed.

This showed that the weight of the dried stony substance in various species of reef corals is on an average about 0.8 that of the same coral heads when alive. Various *Acropora* ranged from 0.7 to 0.9, while branched *Porites* were about 0.8, and large massive *Porites* slightly above 0.9.

Applying this correction, we find that on these Samoan reefs the gain in weight of stony substance per year in various coral heads of average size appears to be as follows:

| | |
|---|---|
| *Acropora* | 19 ounces avoirdupois, or 539 grams per annum |
| *Porites* | 8 ounces avoirdupois, or 227 grams per annum |
| *Pocillopora* | 10 ounces avoirdupois, or 283 grams per annum |
| *Pavona* | 12 ounces avoirdupois, or 340 grams per annum |
| *Psammocora* | 3 ounces avoirdupois, or 85 grams per annum |

Knowing the number of coral heads of these genera upon a reef-flat, we have a means for ascertaining the weight of limestone added to the reef each year by the growth of coral upon its upper surface.

By counting the numbers of coral heads and species of corals on squares of 24 feet on the side (576 square feet), the squares being staked out from 50 to 100 feet apart from the shore to the seaward edge of the reef-flat, we are led to the following conclusions:

On the upper surface of the Aua reef-flat between Breaker Point, Pago Pago Harbor, and the southern end of Aua Village, the area of the reef being 2,550,000 square feet, the number of corals are approximately as follows:

There are about 502,200 heads of *Porites* growing over 72% of the area of the upper surface of the reef-flat, and contributing (due to their growth) per annum 251,000 pounds of limestone.

347,500 *Acroporas* distributed over 56% of the reef-flat contribute annually 500,400 pounds of limestone.

57,600 heads of *Pocillopora* scattered over the entire reef-flat contribute 31,700 pounds of limestone.

55,900 Psammocora distributed over 30% of the reef-flat contribute each year 11,000 pounds of limestone.

15,500 Pavona found over 35% of the reef-flat area contribute each year about 11,600 pounds of limestone.

Thus the growth of these corals appears to add about 805,000 pounds of limestone each year to the upper surface of the reef-flat and as these genera contribute 95% of the entire coral heads of the reef-flat, it is probable that about 840,000 pounds or 380,500 kilograms of limestone is added to the coral reef each year by the growth of the corals over its surface.

There are, however, certain obvious losses which may be approximately determined. Among these the wash of the breakers due to the prevailing trade wind drives a current of about 40 feet per minute over the surface of the reef-flat from Breaker Point, northward to the jagged northern edge of the reef-flat over which the drifted limestone sand spills into deep water.

In order to determine the annual loss of loose limestone sand lost to the reef-flat by being drifted off its northern edge, 6 barrels each 2 feet in diameter were weighted with volcanic rocks and sunk off the reef-flat close to the northern edge so that the water was about 1 foot deep over their open tops at lowest tide.

It was found that on an average each barrel caught 0.7 of a pound of limestone sand per day; the sand being dehydrated in alcohol and dried in the sun before being weighed.

Further tests showed that the barrels caught on an average only 12% of the sand which was carried in the current over their open tops from the edge of the reef-flat.

The northern edge of the reef-flat is about 1200 feet wide, but sand spills over its edge along only about 800 feet of this length, the 400 outermost feet of reef edge being subjected to the constant inward wash of the waves.

Using these data, it appears that about 100,000 pounds of sand are washed off from the reef-flat by currents each year.

There are 290,000 holothurians on the Aua reef-flat between Breaker Point and the edge of the reef off Aus Village; and as the acidity of their gastric cavities appears to be the same as that of the holothurians of Florida, each one, according to experiments made at Tortugas in 1917, might be expected to dissolve 10 pounds of sand per annum; or 2,900,000 pounds of sand may be destroyed annually over this reef-flat by solution due to holothurians.

Thus the corals appear to add annually about 840,000 pounds of limestone to the reef, but 3,000,000 pounds, or nearly 3½ times this weight of limestone appears to be removed annually by holothurians and by currents, and other factors such as boring algae, mollusca and fishes; the efficacy of which we have not been able to calculate, add still more to the destruction of limestone although their influence is in some measure offset by the growth of nullipore algae, and lithothamnion. Alcyonaria although a decided factor in some reefs are so rare as to be negligible over the Aua reef-flat.

In most respects this Aua reef is a typical average Pacific fringing reef and our data appear to explain the disappearance of the lithothamnion ridge over the shoreward parts of the reef-flat as the reef grows outward. Moreover, the reef-flat appears to be deepening at present although the average depth of water over it at low spring tide is less than one foot.

This does not mean that such a fringing reef must necessarily change into a barrier reef through waste and solution of limestone from its floor, for as the reef-flat deepens the currents must lessen, and a sandy or muddy bottom, while unsuited to the growth of coral, is also not so favorable for holothurians as are the small patches of limestone sand surrounded by dead and living coral such as characterizes the Aua reef-flat today.

Thus as the reef-flat deepens, the factors which degrade it probably diminish and a balance may be attained between the accession of limestone due to growth of corals and other organisms and its loss due to mechanical and organic causes.

It is interesting to see that although *Porites* heads are more numerous than *Acropora*, yet *Acropora*, due to its remarkable growth-rate, is the most important coral genus in building up the Pacific reefs.

These results account for the disappearance of the lithothamnion ridge as the reef grows outward. This ridge is formed only on the breaker-washed seaward edge of the reef and it projects somewhat above low tide level, but as the reef extends seaward the ridge disappears over the shoreward parts of the reef-flat.

The growth-rate of these Samoan corals is nearly twice as rapid as is that of similar genera in Florida and the Bahamas, the growth rate of Atlantic corals being well determined due to the exhaustive studies of Vaughan.

This is probably due to the fact that in the Atlantic the corals live chiefly on the outer edges of shallow limestone flats the muddy bottoms of which become churned up by the waves, and pelagic life is largely killed in the water over the flats. Thus in the Atlantic, the corals are well fed only on the incoming tide, very little reaching them from the water passing outward from the flats.

In the Pacific, on the other hand, the lagoons are generally deeper than in the Atlantic and there is but little limestone mud so that the corals receive food both from the rising and from the ebbing tide; and, as is well known, the growth-rate of coelenterates is a factor of their food supply.

The following table shows the average increase in dimensions of corals from Samoa, and from the Florida-Bahama region; the growth-rate of the Atlantic genera being taken from the report by T. W. Vaughan, 1915, *Year Book of the Carnegie Institution of Washington*, No. 14, pp. 227–228.

| GENUS OF CORAL, AND LOCALITY | AVERAGE INCREASE IN DIAMETER | NUMBER OF RECORDS | AVERAGE INCREASE IN HEIGHT | NUMBER OF RECORDS |
|---|---|---|---|---|
| | *mm.* | | *mm.* | |
| Acropora from Samoa | 98 | 21 | 48 | 21 |
| From the Atlantic according to Vaughan* | 54 | 71 | 35 | 42 |
| Branched Porites from Samoa | 47 | 6 | 25 | 6 |
| From the Atlantic according to Vaughan | 26 | 207 | 19 | 106 |
| Massive Porites from Samoa | 35 | 9 | | |
| From the Atlantic according to Vaughan | 13 | 120 | | |

* Vaughan's estimate is based upon branching species such as *Acropora cervicornis; A. prolifera* and *A. palmata*. My records of somewhat similar species in Samoa show an average increase in width of 178 mm. and 79 mm. in height. Many of the Samoan *Acroporas* are low, encrusting or dome-shaped forms such as are not found in the Atlantic.

It will be observed that on the average the 21 Samoan *Acroporas* each gained 539 grams in weight per annum. This appears to be about 3 times as great as the annual increase in weight of *Acropora palmata*, of the Bahamas, for Vaughan 1915 *loc. cit.* p. 230 gives this as 173 grams per annum.

The final paper of which this is a preliminary abstract will be published by the Carnegie Institution of Washington.

# THE DISTANCES OF SIX PLANETARY NEBULAE

By Adriaan van Maanen

Mount Wilson Solar Observatory, Carnegie Institution of Washington

Communicated by W. S. Adams, November 7, 1918

In a previous note to these Proceedings[1] the parallax was published for the planetary nebula N.G.C. 7662. Attention was called to the scarcity of material then existing for the distances of the nebulae in general. The work has since been taken up with the 60-inch reflector of the Mount Wilson Observatory (the equivalent focal length of 80 feet being used), and in all, the parallaxes of eight nebulae have now been determined; six of these are planetaries, while the other two are spirals; the full details for the latter have been published in *Mount Wilson Contributions*, No. 158.

The images of the central stars were even better adapted to accurate measurement in the case of the five other planetaries measured after N.G.C. 7662 than on the plates of this object; here the nebulous ring was close to the central star and of considerable density; in all others, however, the nebula is either so much fainter than the central star, as in N.G.C. 2392, 6804, 6905, and 7008, that it is hardly visible on the plates, or it is at such a distance from the central star, as in N.G.C. 6720, that the nebula is not likely to interfere with measuring the central images. For this reason the existence of a large systematic error in the parallaxes is very improbable; other conditions also favor the belief that any systematic error will be small. As all the fields are near the Milky Way we have an abundance of comparison stars close to the central stars; in no case was the distance greater than seven minutes of arc; the central stars have in all cases the appearance of normal stars and are measurable with great accuracy.

N.G.C. 2392 is relatively bright; on that account a rotating sector with an opening of 36 degrees was used in order to make the central star comparable in magnitude with the faint stars which are preferably to be used for comparable purposes.

In table 1 the observational material is collected for the fields of all six planetaries.

The central star of N.G.C. 6720 is so faint that exposures of 35 or 40 minutes might have been better; fortunately, for this nebula another reliable parallax determination is available—that by Newkirk, who has derived from 74 plates taken with the Crossley reflector of the Lick Observatory a parallax of $+0''.015 \pm 0''.007$.[2] In the following discussion the weighted mean, viz., $+0''.008$, has been used for the absolute parallax.

To derive a homogeneous system for the photographic magnitudes of the central stars, counts were made of the number of stars of equal and brighter magnitudes in as large an area as the plates would allow; then with the help of Table IV of *Publications of the Astronomical Laboratory at Groningen*, No. 27, the apparent photographic magnitudes, $m$, were derived.

The values are given in the third column of table 2; by means of the formula $M = m + 5 + 5 \log \pi$, we can derive the absolute magnitudes $M$, which are given in the fourth column.

The mean absolute magnitude is +9.1 and the deviations from this mean are small; this may be due to a small dispersion or to the choice of objects; the material, however, is still insufficient to discern which of the two possibilities is the cause.

This faint absolute magnitude, +9.1, is noteworthy because the spectra of these objects consist in many cases mainly of bright lines, whereas, with the stars at large, bright-line spectra are usually associated with high luminosities; for the Wolf-Rayet stars for instance, whose spectra resemble those of the planetaries in many respects, we find a mean absolute magnitude not far from 0.[3] On the other hand, assuming the relation between luminosity and radial velocity found for the stars to apply to the planetary nebulae, the high radial velocities observed for these objects, about 29 km. per second,[4] are in good agreement with their low intrinsic brightness. Thus, for 13 K and M stars of mean absolute magnitude 10.0, Adams and Strömberg found a mean radial velocity of 30 km. per second.[5]

TABLE 1

| N. G. C. | EXPOSURE TIME | NUMBER EXPOSURES | NUMBER OF COMP. STARS | $\pi$ REL. | P. E. |
|---|---|---|---|---|---|
| 2392 | $15^m$ | 18 | 8 | +0.″020 | ±0.″003 |
| 6720 | 25 | 14 | 9 | +0.002 | 0.005 |
| 6804 | 25 | 18 | 8 | +0.020 | 0.003 |
| 6905 | 20 | 18 | 9 | +0.013 | 0.002 |
| 7008 | 20 | 20 | 9 | +0.014 | 0.004 |
| 7662 | 25 | 16 | 8 | +0.021 | ±0.004 |

TABLE 2

| N. G. C. | $\pi$ ABS. | $m$ | $M$ |
|---|---|---|---|
| 2392 | +0.″022 | 10.0 | + 6.7 |
| 6720 | +0.008 | 14.7 | + 9.2 |
| 6804 | +0.022 | 13.4 | +10.1 |
| 6905 | +0.015 | 14.5 | +10.4 |
| 7008 | +0.016 | 12.8 | + 8.8 |
| 7662 | +0.023 | 12.9 | + 9.7 |

One class of stars, which seems to take an intermediate position, is that of the novae, whose spectra pass through the planetary spectrum towards that of the Wolf-Rayet stars. The parallax-material, which is extremely scarce as yet and includes only 3 novae, points to absolutely faint magnitudes, in the mean +6.9, for these stars when at their minimum luster. The radial velocities, however, for the three cases in which they have been determined, are small, in the mean 10 km. per second; this would indicate high luminosities. It should not be forgotten, however, that the relation between luminosity and radial velocity is based on stars of the same spectral type and it is questionable if this relation may safely be applied to stars of all types, including planetaries and novae.

The appearance of the nebulosity around Nova Persei and especially the little nebulous ring, which was photographed by Ritchey[6] and Pease[7] in 1917, opens the way for speculations concerning a possible relation between novae and planetary nebulae. To arrive at conclusions, however, further observational data are necessary.

The size of the planetary nebulae can be deduced from the above mentioned parallaxes, by which the diameters measured in arc can be converted into astronomical units or into light-years; the results are given in table 3; for the diameters in this table the major axes have been used.

TABLE 3

| N. G. C. | DIAMETER | | |
|---|---|---|---|
| | Angular | In astronomical units | In light-years |
| 2392 | 46″ | 2,100 | 0.03 |
| 6720 | 80 | 10,000 | 0.16 |
| 6804 | 32 | 1,450 | 0.02 |
| 6905 | 47 | 3,100 | 0.05 |
| 7008 | 95 | 5,900 | 0.09 |
| 7662 | 31 | 1,350 | 0.02 |

By comparing these values with the size of our solar system (the diameter of the orbit of Neptune is 60 astronomical units) it is clear that the planetary nebulae must be of enormous size.

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[1] Maanen, A. van, these PROCEEDINGS, **3**, 1917, (133–136).

[2] Newkirk, B. L., *Lick Obs. Bul.*, *Univ. Cal.*, *Berkeley*, **9**, 1917, (100–107).

[3] Maanen, A. van, *Pub. Astron. Soc. Pacific*, *San Francisco*, **30**, 1918, (308).

[4] Campbell, W. W., and Moore, J. H., these PROCEEDINGS. **1**, 1915, (496–498).

[5] Ritchey, G. W., *Pub. Astron. Soc. Pac.*, *San Francisco*, *Cal.*, **29**, 1917, (256).

[6] Adams, W. S., and Strömberg, G., *Mt. Wilson Contr.*, No. 131; *Astroph. J.*, *Chicago*, **45**, 1917, (293–305).

[7] Pease, F. G., *Ibid.*, **29**, 1917, (256–257).

## *NATIONAL RESEARCH COUNCIL*

### MINUTES OF THE MEETING OF THE EXECUTIVE BOARD

At the National Research Council Building, July 9, 1918, at 9.40 a.m.

Present: Messrs. Bogert, Cross, Dunn, Johnston, Kellogg, Merriam, Millikan, Noyes, Pearce, Woodward, and by invitation, Mr. Edgar.

The minutes of the preceding meeting, April 30, 1918, having been circulated by mail, were considered and approved.

The Acting Chairman, Mr. Noyes, reviewed the activities of the Council since the last meeting of the Executive Board, April 30, 1918, stating that the President of the United States had issued an Executive Order defining the functions of the Council, that the Carnegie Corporation of New York had made an appropriation of $100,000 for the expenses of the divisions, and that the President of the United States had allotted, through the Council of National Defense $61,000 for the running expenses of the Council, including the Research Information Committee, for the period from July 1 to December 31, 1918. He stated further that the work of the Research Information Committee had been increasing steadily, and that the third office had been established in Rome. He also briefly described the plan for the endowment of research in chemistry and physics which had been submitted to the Rockefeller Foundation.

The National Research Council budget was presented, and Mr. Dunn

*Moved:* That the allotments in the budget as presented be approved. (*Adopted.*)

*Moved:* That there be presented at each monthly meeting of the Executive Board a *budget sheet* showing for comparison the following items: Funds Available for the year, Total Amounts allotted, Liabilities, Expenses, and Balances. (*Adopted.*)

Motion adopted at the meeting of the Interim Committee, July 3, 1918: That the Carnegie Institution of Washington be requested to allow Mr. John L. Wirt, its bursar, to advise the National Research Council in regard to an improved accounting system for the carrying on of its business and that the Secretary and Treasurer be asked to confer with Dr. R. S. Woodward and Mr. Wirt in regard to the matter.

Mr. Woodward, being present, offered the services of the accountant of the Carnegie Institution of Washington, Mr. John L. Wirt and his assistant, to look into the accounts of the National Research Council and to make suggestions as to improved methods of carrying them on. The offer was accepted.

The National Research Council accepted full membership in the Emergency Council on Education, and J. C. Merriam was elected representative of the Council. The membership fee of $100 to cover expenses was authorized.

The President of the National Academy of Sciences has appointed as members of the National Research Council the following men, upon the recommendation of the Interim Committee:

Brig.-General Marlborough Churchill, Military Intelligence Division, U.S.A.
Major-General Wm. L. Sibert, Director, Chemical Warfare Service, U. S. A.
Brig. General Clarence C. Williams, Acting Chief of Ordnance, U. S. A.
Comfort A. Adams, President, American Institute of Electrical Engineers.
Philip N. Moore, Past President, American Institute of Mining Engineers.
A. A. Stevenson, Past President, American Society for Testing Materials.
George S. Webster, American Society of Civil Engineers.

Generals Churchill, Sibert, and Williams automatically become members of the Military Division; Messrs. Adams, Moore, Stevenson, and Webster were elected members of the Engineering Division to represent the Societies as indicated.

Paul Brockett was elected Assistant Secretary of the National Research Council.

The franking privilege was extended to the Committee on Patents, the Camouflage Committee, and the Committee on Neurology and Psychiatry, for official correspondence.

Mr. Hale requested in a telegram that all persons holding office connected with the National Research Council should take the 'oath of office' and the Interim Committee has directed that this be done.

The appointment of the Committee on Nitrate Investigations was reported and it was decided to report to the Executive Board the official authorization of this Committee contained in the following letter:

April 16, 1918.

From: The Acting Chief of Ordnance.
To: Mr. Edward R. Stettinius, Second Assistant Secretary of War.
Subject: Plan for organizing nitrate investigations.

1. Under date of March 30 the National Research Council submitted to you a plan for organizing nitrate investigations. This plan was referred by you to the Ordnance Department and by direction of the acting chief of ordnance was the subject of conference between the officials of the Nitrate Division and Drs. Hale, Johnston, and Noyes of the National Research Council.

2. The following report is submitted as a joint recommendation:

It is our opinion that the best method of securing the desired coöperation is for the Secretary of War to request the National Research Council to designate two or more research men, who shall be appointed Consulting Chemists to the Ordnance Department, and who, together with the officer in charge of the Research Section of the Nitrate Division (now Lieutenant Colonel A. H. White), shall constitute a small executive committee whose duties shall be (a) to survey the researches now in progress, (b) to plan further investigation relating to nitrogen-fixation processes, (c) to arrange for the active prosecution of such investigations, and (d) to exercise close oversight over their progress; the actions taken by this committee and the allotment of funds recommended to it to be subject to the approval of the Chief of the Nitrate Division.

It has been arranged that, if this request be made, the Council will designate as Chairman of the Committee, Arthur A. Noyes, Chairman of the Nitrate Supply Committee appointed by the Secretary of War, and Director of the Research Laboratory of Physical Chemistry at the Massachusetts Institute of Technology; and as second member of the Committee, John Johnston, Executive Secretary of the National Research Council and

Chairman of its Division of Chemistry and Chemical Technology, and Research Chemist of the American Zinc, Lead and Smelting Company. Both these men are resident in Washington and will devote a large part of their time to the work.

For the Nitrate Division

(Signed) J. W. Joyes,
*Col. Ord. U. S. A.*

For National Research Council

(Signed) George E. Hale,
*Chairman,*

(Signed) John Johnston,
*Secretary,*

(Signed) Arthur A. Noyes,
*Chairman of Nitrate Committee.*

I recommend that this joint report be approved:

(Signed) W. S. Peirce,
*Brig. Gen. Ord. N. L. Acting Chief of Ordnance.*

1st Ind.
Office of Second Assistant Secretary of War. To the Acting Chief of Ordnance.
1. Forwarded. Approved.

(Signed) Edw. R. Stettinius,
*Second Assistant Secretary of War.*

*Division of General Relations.*—The name of the Administrative Division was changed to Division of General Relations (including Foreign Relations, State Council Relations, Industrial Relations, and Relations with Educational Institutions). The Chairman gave a general account of its recent activities.

*Committee on Relations with Educational Institutions.*—The Chairman, Mr. Merriam, said that at the request of the Committee on Education and Special Training, the question of the reorganization of the courses in colleges and universities to meet war purposes had been considered, and that the Division of Geology and Geography had formulated plans for the reorganization of methods of presenting new courses or modifications of courses in the fields covered by that Division. It was

*Moved:* That the Council express an interest in the scheme which the Committee on Education and Special Training has in preparation and a willingness to assist the Committee in such ways as it may desire. *(Adopted.)*

*Research Information Committee.*—S. L. G. Knox was named Scientific Attaché of the American Embassy at Rome, and Edgar Buckingham, of the Bureau of Standards, was elected Technical Assistant to the Scientific Attaché at Rome.

Mr. Noyes brought up for consideration the work of the Research Information Committee and presented the following order from the War Department in regard to the part that the Army would take in cooperating with the National Research Council in the securing and distributing of information:

WAR DEPARTMENT,
THE ADJUTANT GENERAL'S OFFICE,
WASHINGTON

July 2, 1918

From: The Adjutant General of the Army.
To: The National Research Council, Washington, D. C.
Subject: Information Section, Ordnance Department, and Research Information Committee.

1. The Secretary of War directs that you be informed as follows:

2. The Research Information Committee was formed to establish machinery by means of which the General Staff of the Army and Navy, the scientific organizations in the United States who are working on problems connected with war production and invention, and the various committees of the Council of National Defense charged with work of this nature, may be put in touch with the developments and experimental work being carried on, not only in this country, but in Europe, and kept mutually informed of the state of development of work of this nature.

3. In pursuance of the order of the Secretary of War, establishing this Committee and in order effectively to do this work, it is vitally necessary that the utmost of cordial cooperation be shown by each of the bureaus and committees in question with the Research Information Committee. To secure this the following is directed:

(a) All military Bureaus requiring scientific and technical information are given official status on the Research Information Committee in Washington, D. C.

(b) Representatives of Military Bureaus or of research committees collecting information abroad will be instructed, by their chiefs, to put themselves into direct relationship with the joint committees of the Research Information Committee sitting in Paris or London or later in Rome, in order that information be at once dispatched to the Research Information Committee at Washington, D. C. All communications of scientific investigations or research shall be routed through these channels, even though other channels are employed at the same time.

(c) Official means of intercommunication, such as memorandums, bulletins and the like, between Bureaus of the Army and Committees for research shall be developed to such a degree of efficiency by the Research Information Committee that the distribution of information shall be practically automatic.

(d) Before sending officers or civilians abroad for investigation work, all Army Bureaus or civilian research committees shall get in touch with the Research Information Committee at Washington, D. C. for information and guidance.

(e) The present method of routing information memoranda for file and distribution through the Military Intelligence Branch will not be discontinued.

(f) You will immediately notify this office and the Research Information Committee of the name of the officer who shall represent your Bureau before the Research Information Committee.

By order of the Secretary of War:

(Signed) PAUL GIDDINGS,
*Adjutant General.*

*Moved:* That a medical representative be appointed for the Paris office of the Research Information Committee, provided the expenses can be met from the Medical Division or from outside sources. (*Adopted.*)

*Engineering Division.*—Reference was made to the affiliation with engineering societies and the forming of an advisory board composed of representatives of the chief engineering societies. Ambrose Swasey and John R. Freeman, were elected members of the Division, and W. J. Lester was elected

Vice-Chairman. The following men were elected to membership in the Engineering Division to represent the Societies indicated:

Comfort A. Adams, President, to represent the American Institute of Electrical Engineers.

Philip N. Moore, Past President, to represent the American Institute of Mining Engineers.

A. A. Stevenson, Past President, to represent the American Society for Testing Materials.

George S. Webster, to represent the American Society of Civil Engineers.

*Section on Mechanical Engineering.*—The resignation of S. L. G. Knox as Chairman of this Section was accepted with regret, and W. J. Lester was elected Chairman in his place.

*Section on Tanks.*—A. J. Mason was elected Chairman of this Section.

*Section on Metallurgy.*—W. P. Barba, G. W. Sargent of the Ordnance Department, and J. E. Johnson, Jr. have been added to this Section.

*Section on Electrical Engineering.*—C. A. Adams was elected Chairman of this Section.

*Engineering Committee.*—Mr. Dunn asked if the old Engineering Committee had been in existence or was in existence since War Organization was adopted on March 9, 1918. After discussion it was *Moved:* That notices be sent to the members of the former Engineering Committee by the former Chairman, advising them of the new war organization under plans of March 9, 1918, and inviting their continued cooperation to such an extent as the time during which they can be present in Washington will permit. *Adopted.*

Mr. Dunn stated that while he was Chairman of the Engineering Committee, he had asked the J. G. White Engineering Corporation of New York, to prepare for the National Research Council an elaborate paper on "Clean Coal vs. Dirty Coal," and that a well-illustrated pamphlet had been prepared and printed; of which about 100 copies had been distributed. The plates having been unfortunately destroyed, he now asked authority to reprint the pamphlet, and it was:

> *Moved:* That the J. G. White Engineering Corporation be given permission to reprint the Clean Coal Report of the Engineering Committee of the National Research Council, provided credit be given to the National Research Council for the report. (*Adopted.*)

*Division of Chemistry and Chemical Technology.*—Since the last report the Executive Committee of the Division has been constituted, as follows:

*John Johnston, Chairman.

Samuel Avery, Vice-Chairman.

Graham Edgar, A. A. Noyes, E. W. Washburn, National Research Council.

W. D. Bancroft, E. P. Kohler, A. B. Lamb, American University Experiment Station.

*C. L. Alsberg, Bureau of Chemistry.

C. L. Parsons, Bureau of Mines.

W. F. Hillebrand, Bureau of Standards.
M. T. Bogert, Chemical Warfare Service, N. A.
H. D. Gibbs, U. S. Signal Corps.
S. A. Tucker, War Industries Board.
*Willis R. Whitney, Naval Consulting Board.

* Nominated to membership by American Chemical Society.

The Division is holding regular meetings lasting for about half an hour on Monday, Wednesday and Friday morning of each week, at which matters of interest are brought up as they arise.

The Division organized a conference on the whole question of explosives, which was attended by representatives of the Army, the Navy, certain civil bureaus of the Government, and by representatives of the large explosives firms. As a result of this conference, it was arranged that an active committee should be appointed to take charge of this whole matter, and accordingly a Committee on Explosives Investigations is being organized. The constitution of this Committee is:

Charles E. Munroe, Chairman.
W. C. Spruance, Jr., Lieutenant Colonel, Ordnance Department.
L. L. Summers, War Industries Board.
T. S. Wilkinson Lieutenant Commander, U. S. N.

E. W. Washburn of the University of Illinois was elected a member of the Chemistry Division; he is especially interested in the question of allocating research problems to men still at the universities and has already made substantial progress in this matter.

*Committee on Gases Used in Warfare.*—It seemed to be the opinion of the meeting that this Committee may now be discontinued since the present status of this whole work and its absorption by the War Department leaves little or nothing for the Committee to do; but it was thought that no action should be taken in this direction until Mr. Manning's recommendation should be received.

*Moved:* That the National Research Council express to the Committee on Gases Used in Warfare its high appreciation of the work initiated by that Committee and ably carried out through the efforts of its Chairman, Mr. Van H. Manning, Director of the Bureau of Mines; and thank the Committee for their work. (*Adopted.*)

*Division of Geology and Geography.*—The Chairman of the Division, Mr. Merriam, reported that Isaiah Bowman, Director of the American Geographic Society, had been elected a member of the Executive Committee, and J. E. Spurr, a member of the Division. The following two books have been recommended for inclusion in the official list of military books:

W. M. Davis, Handbook of Northern France.
D. W. Johnson, Topography and Strategy in the War.

The following subjects have been considered and acted upon by the Division:

Course of map making at Columbia University.
Lectures on Geography of Northern France.
Card system for personnel in geology and geography available in this country for special kinds of service.
Assistance in selection of camp sites.
Road material report (deposited in U. S. Geological Survey).
Army map instruction in relation to armies in the field.
Needs of war service to geologists.
Instruction in training camps on the subject of Geology and Geography.
Exchange of information bearing on geologic and geographic work.
Coöperation with Committee on Scientific Research of California.
Resources of clear quartz crystals available in this country.
Instruction in geology and geography in educational institutions (in charge of a special committee as follows: C. K. Leith, Chairman, R. A. F. Penrose, Lawrence Martin, Isaiah Bowman, E. B. Matthews.)

*Division of Medicine and Related Sciences.*—The Chairman of this Division, Mr. Pearce, after reviewing the activities of the Executive Committee, reported progress on the following problems in research work:

The toxicity of Preserved Foods.
Protectors for the ear.
Production of acetone by the fermentation of starch.
Special researches on antiseptics and insecticides.
Louse investigations.
Study of special and rare sugars.
Influence of high explosives in causing shock.
Studies of peripheral nerve injury and repair.
Experimental study of empyema due to streptococcus.
Studies of Anaerobic bacteria.
Studies of hemolytic streptococci occurring in cheese.
Investigation of lung motor.
Study of 13,000 records of soldiers discharged from service on account of nervous or mental trouble.
Discovery of metal or other substitute for petri dish.
Study of Hemostatics.
Investigation of edema among infants
Problems of munition workers.
Critical study of Dreyer's method of testing agglutine of typhoid.
Under the *Sub-Committee on Physiology:*
Study of the Physiology of Shock.
Respiratory Regulation of the $CO_2$ Capacity of the Blood.
Under the *Sub-Committee on Physiological Chemistry:*
A study of the availability of velvet beans.
The use of malt sugar as a substitute for cane sugar.
The substitution for acetone for various therapeutic products.

*Committee on Intoxications among Munition Workers.*—The present membership of this Committee is as follows:

H. Gideon Wells, University of Chicago, Chairman.
Alice Hamilton, Bureau of Labor Statistics.
David L. Edsall, Massachusetts General Hospital, Boston, Mass.
A. D. Reilley, Captain, Ordnance Department.

*Committee on the Physiology of Shock.*—Elected to membership on this Committee:

G. N. Stewart, of Western Reserve University.

*Committee on Neurology and Psychiatry.*—The name of this Committee was changed to "Committee on Neurology and Psychiatry;" and E. E. Southard of the State Psychopathic Hospital of Massachusetts was appointed Acting Chairman with authority to reorganize the Committee; the present Chairman, Stewart Paton, now in active military service, remaining nominally as Chairman.

*Committee on Toxicity of Preserved Foods.*—The following names were approved as members of this Committee:

J. J. Abel, Professor of Pharmacology, Johns Hopkins Medical School, Chairman.

M. J. Rosenau, Professor of Preventive Medicine, Harvard Medical School.

*Sub-Committee on the Study of Fatigue in Relation to Industrial Pursuits.*—Upon recommendation of the Chairman, F. S. Lee, the membership of this Committee was completed by adding the members of the Committee on Industrial Fatigue of the Division of Labor of the Council of National Defense, the Committee as enlarged being as follows:

Frederic S. Lee, Professor of Physiology, Columbia University, Chairman.

Thomas Darlington, Secretary Welfare Committee, American Iron and Steel Institute.

Robert E. Chaddock, Associate Professor of Statistics, Columbia University.

Raymond Dodge, Professor of Psychology, Wesleyan University.

David L. Edsall, Professor of Clinical Medicine, Harvard Medical School.

P. Sargent Florence, Organizing Secretary of the Committee of the British Association for the Advancement of Science, appointed to investigate Fatigue from the Economic Standpoint.

Josephine Goldmark, Publication Secretary, National Consumers' League.

Ernest G. Martin, Professor of Physiology, Leland Stanford University.

J. W. Schereschewsky, Officer in Charge of Field Investigations in Occupational Diseases and Industrial Hygiene, Public Health Service.

Ernest L. Scott, Associate in Physiology, Columbia University, Captain Sanitary Corps, U. S. A.

A. W. Ryan.

*Division of Agriculture, etc.*—The Chairman of the Division, Mr. Kellogg, reported that A. F. Woods, of Maryland State Agricultural College, has been elected Chairman of the Committee on Agriculture, and Vice Chairman of the Division; and that the following problems were under consideration: Tropical Hardwoods, Fish Preserving, Rodent Pests.

*Special Committee on Sisal and Sisal Substitution.*—This committee has been formed for the consideration of the problem of sisal and sisal substitution for use as binder twine, with E. M. East, Bussey Institution, as Chairman.

*Special Committee on Fertilizers.*—The following members have been appointed on this Committee and a plan for publicity work to educate the people on the subject has been formulated:

A. F. Woods, Chairman, E. W. Allen, F. J. Alway, S. Avery, K. F. Kellerman, W. G. Lipman, B. E. Livingston.

*Moved:* That the Special Committee on Fertilizers be allotted the sum of $1,000 or such part thereof as may be necessary to meet the general expenses of that Committee, from the allotment for the Division of Agriculture. (*Adopted.*)

*Moved:* That the Special Committee to undertake a determination of Physiological Salt Requirements of certain Cultivated Plants be allotted an appropriation of $2250 or such part thereof as may be necessary for the general expenses of that Committee, from the allotment of the Division of Agriculture. (*Adopted.*)

*Moved:* That the Special Committee on Problems of Protein Metabolism in Animal Feeding be allotted $500 for current expenses from the allotment of the Division of Agriculture. (*Adopted.*)

Mr. Woodward *Moved:* That the above acts of the Interim Committee be approved, ratified and confirmed. (*Adopted.*)

The Board adjourned at 12.45 a.m.

PAUL BROCKETT, *Assistant Secretary.*

## MINUTES OF THE MEETING OF THE EXECUTIVE BOARD

AT THE NATIONAL RESEARCH COUNCIL BUILDING, AUGUST 13, 1918, AT 9.35 A.M.

Present: Messrs. Dunn, Mendenhall, Merriam, Noyes, Southard, Washburn, Welch, Woods, Woodward, and Yerkes.

The Acting Chairman reviewed the work of the Research Council since the last meeting; a general statement of the activities of some of the Divisions is appended.

The holding of a joint meeting of the Executive Board with the Council of the National Academy on Monday, September 9, to consider matters to be presented to the conference of Academies in London on October 9, was approved.

*Committee on Reconstruction Problems.*—Mr. Merriam presented a report from the special committee brought together to consider plans for reconstruction work after the war.

*Moved:* That the following recommendations of the Committee be approved:

That a committee be constituted on Reconstruction Problems, to be appointed by the Acting Chairman.

That this committee be empowered to increase its membership by associating with itself experts in each line investigated and to coöperate with other organizations engaged in reconstruction work.

That an appropriation of $5,000 be made to cover necessary clerical and traveling expenses, procuring of documents, and reports, provided funds are available.

That the committee be instructed to map out the whole field of reconstruction, and ascertain what agencies are now working in each portion of the field.

That the committee make plans for research into important reconstruction problems, within the scope of the activities of the National Research Council not now being covered and to indicate the agencies which should undertake this work. (*Adopted.*)

The Chairman read the following letter of July 25, 1918, from Ralph Barton Perry, Executive Secretary of the Committee on Education and Special Training of the War Department to the Acting Chairman, Mr. Noyes:

I beg to submit the following vote passed at a meeting of the Executive Committee on College Training, held Tuesday, July 23, 1918:

"*Voted* to invite the National Research Council to form a committee for the purpose of reviewing cases of research men who may be candidates for the Enlisted Reserve Corps."

*Moved:* That the Chairman of the National Research Council be authorized to appoint a committee of five, of which the Chairman shall be a member, to recommend to the Committee on Education and Special Training men of draft age engaged in research of such importance that it seems desirable to have them assigned to scientific work rather than to purely military service. (*Adopted.*)

The Chairman appointed the following committee: J. C. Merriam, Chairman, R. A. Millikan, R. M. Pearce, Vernon Kellogg, A. A. Noyes.

*Section on Relations with Educational Institutions and State Committees:*—

*Resolved:* That the administration of the Section on Relations with Educational Institutions and State Committees be extended by arranging the work for the United States so that it will be related to four districts representing approximately the North Atlantic, Southern, North Central, and Pacific Coast-Rocky Mountain regions, and that under the direction of this Section a representative resident in each of these regions be appointed to confer with Chairmen of local committees, to promote the interests of research, and to care for the relations between the Research Council and the local committees. (*Adopted.*)

*Division of Chemistry and Chemical Technology.*—At the preceding meeting of the Executive Board the matter of the discontinuance of the Committee on Gases Used in Warfare was taken up, but it was decided to defer definite action until a recommendation from Mr. Van H. Manning, Chairman of the Committee, should be received. Mr. Manning wrote recommending that this Committee be discontinued, and it was

*Moved:* That in accordance with the recommendation of Mr. Manning, the Committee on Gases Used in Warfare be dissolved. (*Adopted.*)

The meeting adjourned at 11.45 a.m.

Paul Brockett, *Assistant Secretary.*

General statements as to the recent activities of some of the Divisions are appended.

*Division of General Relations.—Section on Relations with Educational Institutions and State Committees.* The Section has secured and transmitted reports to the War Department Committee on Education and Special Training, on the matter of instruction in scientific subjects with a view to making a recommendation as to the best organization of courses to meet emergency needs. These reports have been discussed with the members of the above Committee and they will be transmitted to the educational institutions with the recommendations of this Committee relating to the organization of instruction for the coming year. The Division of Geology and Geography is working on the preparation of a text book and syllabus to follow the report transmitted and it will probably be desirable for other Divisions of the Council to prepare similar detailed reports on instruction for emergency work.

The Section has been receiving information from the research committees in educational institutions and now has sufficient data to enable it to prepare a preliminary report on the present state of emergency research in the universities and colleges of the country. It has also kept in close touch with the American Emergency Council on Education which is doing important work in bringing together the educational institutions of the country. Of special importance at the present time is the organization of a group of prominent educators to consider the reconstruction policy of this country, a question which involves the matter of research in educational institutions.

*Division of Engineering.—Section on Mechanical Engineering.* This Section has taken possession of the machine shop and laboratory of the Carnegie Institute of Technology at Pittsburgh for its work. It has about seven men now at work there and expects to increase their number to twenty or thirty very soon. The Section is preparing to develop several new types of light engines, and to experiment on the more efficient use of gaseous fuel.

*Section on Metallurgy.* This Section is arranging for the manufacture and ballistic testing of lots of several hundred each of helmets made from five types of steel, which in the preliminary tests have given the greatest promise. It has made encouraging tests with some new types of gun. In coöperation with the Bureau of Standards, Bureau of Mines and the Geophysical Laboratory it is making investigations looking towards the economy of manganese in ore-smelting and in steel-making, with the purpose of releasing vessels now used in bringing mangenese ore from abroad. It is arranging to coöperate with the Production Division of the Ordnance Department in standardizing the manufacture of fire-arms. In connection with the Section of Electrical Engineering and the Emergency Fleet Corporation it is seeking to improve the electric welding of ships.

*Division of Chemistry and Chemical Technology.—Changes in the Executive Committee of the Division.* Since the last report of the Division the following changes have occurred in the Executive Committee: Dr. Samuel Avery has resigned as Vice-Chairman and Dr. E. W. Washburn has been appointed to fill the vacancy. At the direction of Colonel Bogert, Majors Avery and Rogers of the Chemical Warfare Service have been assigned to represent this service at the meetings of the Executive Committee. Owing to the resignation of Professor S. A. Tucker from the War Industries Board a vacancy has been created in the Executive Committee of the Division which has not yet been filled.

*Activities of the Sub-Committees.* The following sub-committees of the Division have been especially active: (1) The Committee on Synthetic Drugs; (2) The Committee on Ceramic Chemistry; (3) The Committee on Glue and Other Colloids.

The Committee on Synthetic Drugs has been in close coöperation with the Federal Trade Commission, has initiated several investigations dealing with the manufacture of special drugs, and has arranged to have a number of these drugs recently unobtainable manufactured by American firms.

The Committee on Ceramic Chemistry is engaged in the preparation of two manuscripts dealing respectively with (a) the formation of an organization for coöperative research in the subject of refractory materials, and (b) the formation of a similar organization for the investigation of the washing, blending, and preparation of clays. Under the auspices of this committee a conference was held in Pittsburgh in June, and an organization formed for undertaking a coöperative survey of the ceramic raw materials of the country. This cooperative survey has been inaugurated and is being carried out under the direction of the American Association of State Geologists.

The Committee on Glue and Other Colloids has taken up the question of the standardization of Government specifications for glue.

The Sub-Committee on the Chemistry of Soils and Fertilizers has been discharged and a new fertilizer committee organized under the Division of Agriculture, but with representatives from both divisions, has been constituted with the following personnel: Messrs. Allen, Alway, Avery, Kellerman, Lippman, Livingston, and Woods, Chairman.

*University Research on War Problems.* The work of collecting and assigning war problems to university laboratories for investigation is proceeding satisfactorily. The Vice-Chairman has established relations with the Chemical Warfare Service, the Surgeon General's Department, the War Industries' Board, the Nitrate Committee, the Explosives Committee, and the United States Signal Corps, for the purpose of securing such problems. Thus far, 25 problems have been formulated and 11 have been assigned.

*The Training of Chemists for Government Service.* At the request of the War Department's Committee on Education and Special Training the Vice-Chairman has undertaken to collect data concerning the numbers of chemists required for Government service, together with data concerning any special training which such chemists should have to fit them for Government work. On the basis of these data recommendations concerning any desirable modifications in or additions to collegiate courses in chemistry will be formulated and transmitted to the colleges and universities.

*International Cooperation in the Publication of Chemical Abstracts.* On the initiative of the Division a conference was held recently in Washington and plans were formulated for the presentation to the English Chemical Societies of a scheme for international coöperation in the collection and publication of chemical abstracts.

*Division of Geology and Geography.*—The work of the Committee on Geologic Service in the field of military operations has been completed. The Secretary of War reports that the geologic service will be included in the work of the Engineer Corps, and General Black, Chief of Engineers, states that the Corps is taking up geologic work both in the United States and abroad. Appreciative reference is made to the work of our Division which seems to have served a good purpose. At the present time the geologic service of the Central Powers is greatly superior to that of America and our Allies and is very effectively employed; but there is no doubt that we shall soon surpass the German army in this field of endeavor.

The committee on suggestions as to inclusion of materials from geography and geology in the training courses for officers and men, after a carefully conducted examination of the field, has made its report to the War Department. This report is very favorably received and will be acted upon in the near future. Much credit for this work is due Dr. C. P. Berkey.

The committee on preparation of recommendations on the best organization of instruction in geology and geography has reported and this statement is in the hands of the Committee on Education and Special Training.

*Division of Agriculture, Botany, Forestry, Zoology and Fisheries.*—In the Agriculture Committee special consideration has been given the problem of fertilizers. This Committee has cooperated with the Fertilizer Committee of the Division of Chemistry. In accordance with the joint recommendation of the two Divisions, the fertilizer work has now been consolidated under the Committee on Agriculture with a Special Committee made up of representatives of the two Divisions. Plans are being formulated to bring about coöperation in some of the larger fertilizer questions facing the country. Representatives of the State Colleges of Montana, North and South Dakota, Minnesota, Wisconsin, Iowa, Nebraska and Kansas were asked to form a special organization coöperating with the National Research Council for the purpose of collaborating in the study of the fertilizer problems of that area with special reference to the most economical use of the large deposits of phosphate rock in Montana in connection with the utilization of the large quantities of sulphuric acid that will be available following the war. It is planned also to study the possible utilization of ammonia compounds which will be available as by-products of war industries. It is planned to develop similar coöperative organizations in the study of other fertilizer problems. A special sub-committee has been organized for certain research work in connection with potash substitutes.

A special Committee on Fibers is making a study of the fibers available for binder twine.

The work in Animal Nutrition is being continued in accordance with the project already approved. Plans for a conference of the collaborators of this project have been formulated and the Committee will meet for a special conference early this fall. The work is proceeding satisfactorily.

The Committee on Zoology has been cooperating with the Department of Agriculture in calling attention to the methods of destroying rats and mice to prevent their depredations on stored food products. Through the Council of National Defense an urgent appeal is being sent to all State Councils to utilize the valuable methods recommended by the Department of Agriculture to fight this pest.

The Division is cooperating with the Division of Geology and Geography in gathering information from various colleges in the country as to modifications of courses of instruction to meet war conditions.

## MINUTES OF JOINT MEETING OF THE EXECUTIVE BOARD AND THE COUNCIL OF THE NATIONAL ACADEMY OF SCIENCES

At the National Research Council Building, September 9, 1918 at 9.50 a.m.

Present: Messrs. Abbott, Dunn, Gregory, Hale, Howe, Johnston, Kellogg, Lester, Manning, Mendenhall, Millikan, Noyes, Pearce, Pupin, Stratton, Yerkes, and by invitation Durand, Flexner, and Gunnell.

The minutes of the preceding meeting, and of the intervening meetings of the Interim Committee, having been circulated by mail, were approved.

In considering the request of Dr. Schuster of the Royal Society as to the utility of the International Catalog of Scientific Literature, Mr. L. C. Gunnell described the present organization of the International Catalog and the difficulties under which it has been operating since the war began.

Mr. Noyes *moved:* That, before continuing the publication of the International Catalog, full consideration be given to the practicability of securing coöperation and avoiding duplication in the whole field, including the preparation of indexes, bibliographies, and abstracts of scientific literature. (*Adopted.*)

*Moved:* That the Chairman of the Research Council, Mr. Hale, act as a member and Chairman of the Research Information Service. (*Adopted.*)

*Moved:* That in the absence of the Chairman, Mr. Noyes, Mr. Merriam act as Chairman of the Division of General Relations and Mr. Johnston as Chairman of the Committee on Nitrate Investigations. (*Adopted.*)

*Moved:* That a Budget Committee of three be appointed by the Chairman to take charge of the accounts of the National Research Council. (*Adopted.*)

The Chairman appointed the following Committee: John Johnston, Chairman, Whitman Cross, R. S. Woodward.

Mr. Hale *moved:* That a small local committee on submarine research work be appointed in California, with headquarters at Pasadena, with Professor Harris J. Ryan, of Stanford University, as Chairman. (*Adopted.*)

The Chairman, Mr. Hale, asked for a discussion of the plans suggested for consideration by the Inter-Allied Conference, to be held in London on October

9 at the request of the Royal Society of London, for the benefit of the delegates present at this meeting.

The meeting adjourned at 12 o'clock.

Paul Brockett, *Assistant Secretary.*

There is appended hereto a brief statement of the activities of some of the Divisions of the National Research Council.

*Division of Engineering.*—The Section of Mechanical Engineering is carrying on its work in its Pittsburgh machine shop and has increased its personnel slightly. It is engaged on problems concerned with the development of a number of devices for the use of aircraft, of special balloons and parachutes, of mechanism for the control of trucks and tractors, and is undertaking a general investigation of fatigue of metals.

The work of the Section on Prime Movers on the special engine has been suspended for lack of funds; a special carburetor is being developed.

In the work of the Section on Metallurgy on manganese saving, cooperation has been established in the investigations which they will make jointly with the Bureau of Standards and the Bureau of Mines; and already ten of the thirteen important smelters of manganese ore and eleven out of the sixteen open hearth steel works approached, have promised to cooperate.

A committee is being formed to recommend the proper procedure in making and treating steel ingots for the better class of uses such as cannon, crank shafts, shells and armor. A second committee will take up the question of devising a pyrometer capable of measuring the temperature of the molten steel in open hearth and electric furnaces. Experimental work on the pressing of steel of various compositions into helmets has been begun.

*Division of Physics, Mathematics, Astronomy and Geophysics.*—A considerable fraction of the problems which are catalogued under the activities of the Physical Science Division of the National Research Council are associated with the work of the Science and Research Department of the Bureau of Aircraft Production. This work is at present carried on by about twenty officers and an equal number of civilian employees of scientific and technical qualifications, in addition to some hundred and twenty enlisted men. This does not include the 500 men who carry on in this country and abroad the work of the Meteorological Service of the Army, which is one of the sections of the Science and Research Department.

The work in this country is carried on at Washington, Langley Field, Baltimore, Pittsburgh, at a number of universities, and at several aviation fields. At Langley Field there is a laboratory which employs some thirty officers and men and keeps some ten airplanes continuously engaged on experimental work. At Washington there are some seventy enlisted men working at the Bureau of Standards under the direction of the officers of the Science and Research Department aided by the officers of the Bureau of Standards. At Baltimore there are a dozen or more men working on signalling and balloon problems under the general direction of Major Wood. At Pittsburgh, where the shops of the Carnegie Institute of Technology have been turned over to the work of the Physics and Engineering Divisions of the National Research Council, there are a dozen or more physicists, engineers, and mechanics working upon the design and construction of apparatus. At Columbia University there are some five or six men working on a special and important problem. Since the final testing is done under the authority of the Division of Military Aeronautics there are always a number of men who are at the aviation fields assisting in the making of final acceptance tests.

The work of the Science and Research Department outlined above covers the problems which come under the authority of the Bureau of Aircraft Production, the Division of Military Aeronautics, and the Signal Corps; but, through the activity of the Physics Committee of the National Research Council, investigations have been initiated which come within the authority of other Bureaus of the Army and the Navy.

Thus, the whole sound ranging work, the investigation of the location of aircraft by sound, and certain other allied investigations were transferred to the authority of the Engineers Corps after they had been gotten well under way by representatives of the Research Council. These representatives still act as an integral part of the groups, and the results of the investigations are reported by the Engineers Corps to the central office of the Research Council; so that the Division of Physical Sciences of the Research Council is still acting in an advisory and stimulating capacity in all these fields, and the problems themselves are discussed in the executive committee meetings of the Division of Physical Sciences.

The Anti-Submarine group, of New London, furnishes another illustration of the way in which these developments take place. The ten scientific men constituting this group were chosen in June, 1917, and their activities financed for a number of months by the Research Council, but the funds for their research work are now supplied entirely by the Navy. The reports of the results of this work are made, however, to the National Research Council, and the Executive Committee of the Division of Physical Sciences is represented in the New London meetings which are held each week.

Precisely similar relations are held in regard to certain problems coming under the authority of the Bureau of Ordnance of the Navy, the Ordnance Department of the Army, and the Coast Artillery Board.

The Executive Committee of the Division of Physical Sciences has met but twice during the past month. It has added a number of new problems to the list of approximately eighty which are receiving the attention of this Division.

Mr. E. F. Nichols, who is Chairman of the Committee on Submarine Mines of the Physics Division, has been employed by the Bureau of Ordnance of the Navy for the fuller development and production of devices relating to Naval Ordnance.

*Division of Chemistry and Chemical Technology.*—In addition to the regular meetings on Wednesday and Friday mornings the Division held a special evening meeting on August 28 for the purpose of discussing the arrangement of courses and recommending curricula for the war training of chemists and chemical engineers; this conference was attended by practically all of those now resident in Washington who are especially interested in chemical education. This matter had been referred to a special committee, the active memers of which were H. P. Talbot and E. W. Washburn, who as a result of a questionnaire, had drawn up sample curricula designed to fit the special conditions now obtaining. It was found that the additional chemists required in the near future for government work will exceed 2000, with the probability that the needs for industrial work will be equally large. Analysis of the government requirements shows that about one-third of the number will need only training sufficient to enable them to carry out routine analytical and control work; the remainder will require a good general training with some degree of special training in one of the following: (a) physical chemistry, including electrochemistry and metallography; (b) organic chemistry, explosives in particular; (c) food and sanitary chemistry; (d) physiological chemistry; (e) chemical engineering. In accordance with the requirements two curricula, one for chemistry and one for chemical engineering, were proposed, for the purpose of training men needed as rapidly as possible; and after detailed consideration and discussion, two such sample curricula were recommended to the special committee of the War Department in charge of the whole matter.

The work of discovering chemical problems arising out of the war and assigning them to men at the universities and colleges is growing rapidly. To date thirty-four problems have been formulated, most of which have already been assigned, with the understanding that the research work will be prosecuted actively and that frequent reports of progress will be submitted to the Division. The list of subjects cover a wide range, from questions connected with chemical warfare to questions of conservation of materials a shortage of which is imminent.

*Division of Geology and Geography.*—In the absence of Messrs. Merriam and Cross, Mr. Gregory has acted as Chairman of the Division and also of the Section on Relations with Educational Institutions and State Committees. The chief work of the Division during August has been as follows:

1. Conferences with the Committee on Education and Special Training of the War Department with regard to plans for instruction in geology and geography for Army Training Units.

2. Conferences with geographers located in and near Washington with regard to geological instruction in universities and Army Camps during the period of the war.

3. Preparation of text books on Military Geology and Topography, Introductory Meteorology, and a syllabus on the Geography of Europe. Intro-

ductory Meteorology, a book of about one hundred and fifty pages, has been written largely by officers of the United States Weather Bureau. In the preparation of Military Geology and Topography the Acting Chairman has had the assistance of geologists and geographers in Government Bureaus and in universities.

4. Suggestions for training in map making and map interpretation in Army Camps, prepared largely by Mr. C. P. Berkey of Columbia University, have been submitted for discussion.

## MINUTES OF THE MEETING OF THE EXECUTIVE BOARD

### AT THE NATIONAL RESEARCH COUNCIL BUILDING OCTOBER 8, 1918, AT 9.45 A.M.

Present: Messrs. Bogert, Cross, Dunn, Howe, Merriam, Millikan, Rous, Washburn, and Woodward.

The minutes of the preceding meeting, and of the intervening meetings of the Interim Committee, having been circulated by mail, were approved.

Mr. Dunn *moved:* That the following elections by the Interim Committee for membership in the National Research Council be recommended to the President of the National Academy of Sciences for appointment as members of the Council: Vice Chairmen, W. J. Lester (Engineering Division), E. W. Washburn (Division of Chemistry), Peyton Rous (Division of Medicine), and A. F. Woods (Division of Agriculture), together with C. E. Munroe, Chairman of the Committee on Explosives Investigations, and W. H. Howell, Acting Chairman of the Physiology Committee. (*Adopted.*)

*Moved:* That the whole matter of membership in the National Research Council be considered at a later meeting of the Board, at which time fuller information as to the appointment of members and recommendation for further appointment will be presented. (*Adopted.*)

*Moved:* That the election of the following as members of the Section on Industrial Research be confirmed: John Johnston, Chairman, L. H. Baekeland, G. K. Burgess, F. G. Cottrell, A. D. Flinn, C. E. K. Mees, Walter Rautenstrauch, W. R. Whitney. (*Adopted.*)

*Moved:* That the election of the following as members of the Advisory Committee of the Division of General Relations be confirmed: Theodore N. Vail, Chairman, Cleveland H. Dodge, George Eastman, E. H. Gary, A. W. Mellon, Pierre S. duPont, H. S. Pritchett, E. W. Rice, Jr., Elihu Root and Ambrose Swasey. (*Adopted.*)

*Moved:* That Peyton Rous be appointed Vice Chairman of the Division of Medicine and Related Sciences, in place of Major Yerkes resigned, and that he also be considered a member of the Executive Committee of said Division. (*Adopted.*)

*Moved:* That the pamphlet showing the present war organization of the Council be printed as of the present date. (*Adopted.*)

*Moved:* That the work of the National Research Council is of such importance to the Government that request might be made for deferred classification of men indispensable to its work; that office employees not engaged in research could not be considered as indispensable; and that cases of married men with families should be allowed to rest on the statement of the questionnaire, unless special circumstances make necessary a request for exemption. (*Adopted.*)

*Moved:* That the action of the Treasurer of the National Academy of Sciences in investing funds of the National Research Council, on deposit with the National Academy of Sciences, in readily negotiable bonds, be approved. (*Adopted.*)

The balance sheet as prepared by the Budget Committee was presented for consideration.

Mr. Dunn *moved:* That the Committee be congratulated on the work which has been done in preparing a statement of the accounts of the Council in such an understandable way and that a vote of thanks be transmitted to the Committee, to the Carnegie Institution of Washington and through that Institution to Mr. R. S. Woodward, Mr. John L. Wirt and Mr. E. A. Varela for the valuable aid which has been rendered in this matter. (*Adopted.*)

*Moved:* That all funds received either for general or special purposes of the National Research Council, with the exception of Government funds disbursed by the Council of National Defense, be handled in one bank account, and that all checks drawn against this bank account be signed by the Treasurer or Assistant Secretary and countersigned by the Secretary or Chief Clerk. (*Adopted.*)

*Moved:* That the Chairman be authorized to appoint a committee of three on Publicity and Publications, with the Secretary as an ex officio member, to consider the matter of preparation of accounts of the work of the Research Council for publication and to make recommendations. (*Adopted.*)

The Chairman appointed as members of the Committee on Publicity and Publications John Johnston, Vernon Kellogg and Walter M. Gilbert.

The meeting adjourned at 11.20 a.m.

PAUL BROCKETT, *Assistant Secretary.*

The following statements may be made relative to the work of Divisions of the Council during the past month:

*Division of Engineering.*—Progress continues in the studies undertaken by the Sections on Mechanical Engineering and Prime Movers, and a new subject of investigation concerning a special motor for aircraft, has been added to the list already under consideration.

In the Section on Metallurgy an additional committee has been appointed for a study of the improvement of metals by treatment at blue heat, under the chairmanship of Prof. Zay Jeffries, Director of Research of the Aluminum Castings Company.

In the Section on Electrical Engineering, important progress continues in the electric welding of ships. In particular the cause of the brittleness found at times in these welds has been discovered. About twelve different types of welded joints aggregating about one hundred samples, have been shipped to the Bureau of Standards for mechanical testing. A sub-committee on the metallurgical aspects of welding under the Chairman, S. W. Miller, has been formed. Investigations on the influence of the composition of the ship plates on the properties of the welded joint have been completed.

*Division of Chemistry.*—The work of procuring and assigning for investigation, war problems in chemistry and chemical technology is proceeding. Up to date forty-five such problems have been received and formulated. There is some uncertainty as to the effect which the inauguration of the Student Army Training Corps at educational institutions is going to have upon this work. There seems to be some danger that in many cases the time of the chemists at the universities is going to be entirely taken up with instructional work, and that they will have no time to devote to research on war problems. It is hoped that in this case it will be possible to make use to an increasing extent of the research laboratories of industrial organizations with such laboratories. One of the war problems received from Chemical Warfare Service and assigned to the Research Laboratory of Physical Chemistry at the Massachusetts Institute of Technology has been progressing very satisfactorily and promises to be of very great value to the work of the Chemical Warfare Service. It may be stated that this problem has been attacked previously by other agencies but without success.

The Division is reorganizing its system of sub-committees on the following basis: Sub-committees which have been active and are doing important work in connection with the war will be retained and reorganized as special committees for certain lines of work. Other sub-committees will be discharged and replaced by a list of special consultants made up in most cases of the chairmen of former sub-committees.

At the request of the United States Fuel Administration, the Committee on Ceramic Chemistry has prepared a synopsis of a course in Chemistry of Combustion and Fuel Engineering for distribution to educational institutions. Up to the present time some thirty requests have been received for this synopsis. The synopsis was prepared by Mr. A. V. Bleininger of the Ceramics Committee and Prof. R. K. Hursch of the University of Illinois.

*Division of Geology and Geography.*—The work of the Division was under the direction of Mr. H. E. Gregory, Acting Chairman, for the first half of the month and under that of the Vice Chairman for the latter half. The duties of Acting Chairman of the Council having fallen on the Chairman of the Division, he requested the Vice Chairman to take charge of the work of the Division for the present. The principal work of the Division during September has been as follows:

1. The three textbooks for use in the S. A. T. C. courses in colleges and universities which have been prepared under the auspices of the Division (*Military Geology and Topography*, *Introductory Meteorology* and the syllabus on the *Geography of Europe*) have been completed and are now in course of publication by the Yale University Press. The completion of the manuscript and the reading of proof, etc., has taken a large amount of time.

2. Outlines of the courses in which these textbooks may be used have been presented to and approved by the Military Committee of the War Department on Education and Special Training. Many conferences have been held by the Chairman and Acting Chairman of the Division with the Military Committee in regard to the courses in geology, geography and topography.

3. Many conferences have been held and a large amount of correspondence

conducted with teachers of these subjects in the S. A. T. C. courses of various universities and colleges.

4. An examination of lantern slide material which may be available for the illustration of lectures in the S. A. T. C. courses on the Geography of Europe has been undertaken by Prof. E. B. Mathews of Johns Hopkins University as a committee of one for the Division, his necessary expenses to be paid from the appropriation for the Division.

5. Work of the Division in directions previously reported has been continued.

*Division of Medicine and Related Sciences.*—An important step taken by the Medical Division during the month was to send abroad Dr. Roger G. Perkins of Cleveland as Medical Associate of the Research Information Service in Paris. Dr. Perkins will inform himself of the medical research now going on in England and France and will submit reports from time to time with a view to correlating and stimulating research activities in this country.

Five hundred dollars was voted for study of the etiology of the so-called Spanish Influenza under the direction of Dr. W. H. Park of New York City.

Working committees were appointed for the improvement of transfusion methods; for the treatment of chronic anaemia; and for research along anatomical lines. Many progress reports have been received and several new researches instituted.

PAUL BROCKETT, *Assistant Secretary.*

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